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Editor:

GILBERT VAN INGEN.

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SESSION, 1898–1899.

The Academy will meet on Monday evenings at 8 o'clock, from October 3d to May 22d, in the rooms of the American Society of Mechanical Engineers, at **12 West 31st Street.**

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NOTE REGARDING PUBLICATIONS

OF THE

NEW YORK ACADEMY OF SCIENCES.

Publication of the **Transactions** of the Academy is discontinued with the issue of Volume XVI, 1898. The matter heretofore printed in the Transactions will be incorporated in the Annals.

The **Annals** (8vo), beginning with Volume XVI, will appear with new forms of typography and arrangement of matter; many changes having been made in the endeavor to facilitate the use of the volume for reference purposes. A volume of the Annals will hereafter coincide with the calendar year and will be issued in three parts. The price per volume is three dollars.

The **Memoirs** in quarto form will be published at irregular intervals. Part I of Volume I has been issued.

[Annals N. Y. Acad. Sci., XI., No. 14, pp. 293 to 368, September 12, 1898.]

THE PHYSIOLOGY OF SECRETION.

Albert Mathews.

(Read April 11, 1898.)

I. INTRODUCTION.

A Criticism of the Secretory-nerve Theory.

Nearly fifty years ago it was suggested by Ludwig[48] that secretion was a function of the gland cells controlled by the activity of special nerve fibres. Upon the gland cell, thus emphasized as the prime factor in secretion, and upon its relation to nerve action, most of the subsequent study of the physiology of secretion has been focussed. This study has unearthed such evidences of the truth of Ludwig's hypothesis that to-day few theories of physiology rest upon a foundation apparently firmer, or are more widely accepted, than the hypothesis of secretory nerves. Indeed, the recent discovery,[6] by means of the Golgi and Ehrlich methylen-blue methods, of the remarkably rich distribution of nerves to glands, and of the endings of these nerves about the gland cells, has seemed the final convincing demonstration of the truth of the theory which so many years ago foretold their existence.

The theory of secretory nerves did not long remain in the simple form suggested by Ludwig, for it soon received, at the hands of Heidenhain, a more complete and definite shape. First seriously worked out by him in 1868[21] the theory was further developed in 1878[22] and took its final form in his great treatise on secretion embodied in Hermann's Handbuch der Physiologie in 1880.[23] The Ludwig-Heidenhain theory, thus crystallized by Heidenhain, has been the lens through which the facts of secretion accumulated from 1868 to the present time, have been viewed. This theory may be briefly stated as follows :

Secretion is a specific function of the gland cells controlled by special secretory nerve fibres, acting directly upon these cells. There are two kinds of these nerve fibres : trophic fibres, which render the cell contents soluble ; and secretory fibres, which diminish the resistance to filtration offered by the lumen end of the cell. In consequence of this decreased resistance, the contents of the cell, which are under high endosmotic pressure, escape into the lumen. At the same time the cell imbibes liquid from the lymph space.

Heidenhain, R. Ueber secretorische und trophische Drüsennerven, Pflüger's Archv. f. d. gesam. Physiologie. Bd. XVII, 1878, pp. 60 and following: "The cell is normally under high endosmotic pressure. On nerve stimulation a molecular rearrangement takes place at the lumen end of the cell, so that the resistance to filtration is diminished and water flows out. This flow may be hastened by contractions of the protoplasm, as Kühne observed in the rabbit's pancreas under the microscope. The tension of the water within the cell being thus diminished, water begins to flow out of the lymph and capillaries into the cell. At the end of stimulation molecules are rearranged, the loss of water by the cell ceases, and secretion stops." "The attractive pull on the water comes from the protoplasm of the outer zone."

Before proceeding with the discussion of the evidence upon which this theory rests, it will make the matter clearer to recall the conception of secretion which the Ludwig-Heidenhain theory supplanted. For some of the facts brought forward by these authors are of value, not as direct evidence of the existence of secretory nerves, but because they disprove an alternative earlier conception. The prevalent conception of secretion, before Ludwig's time, was that liquid driven by intra-capillary pressure filtered out through the gland.[48] The chorda tympani was the principal secretory nerve then known, and it was believed to cause secretion by greatly increasing intra-capillary pressure by contraction of the veins or arterioles. The discovery of the vaso-dilator function of this nerve shortly thereafter by Claude Bernard re-emphasized the possibility of a high intra-capillary pressure being an essential cause of secretion. It is not surprising that many physiologists of that day believed that this striking correspondence between vaso-dilation and secretion could not be accidental, and it was natural for them to refer the secretory power of the nerve to its action on the blood vessels.

The first blows against the theory that the vascular system stood necessarily in a causal relation to secretion were dealt by Ludwig and his pupils. They discovered that stimulation of the upper end of the cut cervical sympathetic nerve caused a secretion from the submaxillary gland of the dog,[48] but this secretion, unlike that due to the chorda, was afterwards found to be accompanied by a pronounced vaso-constriction instead of dilation. They found that the pressure capable of being generated by the saliva flowing from Wharton's duct might considerably surpass

the pressure of the blood even in the carotid artery. They thus demolished, once and for all, the filtration theory. They found, further, that the temperature of the saliva secreted from the dog's submaxillary might surpass by 1.5 °C., the temperature of the blood in the carotid artery,[49] and as final evidence that the chorda tympani could induce secretion independent of the vaso-motor action, they brought forward the observation that stimulation of this nerve still caused a secretion, some minutes after the heart ceased to beat.[61] It is not strange that, in the face of such facts, Ludwig should have felt compelled to assume the secretory activity of the gland cell.

Heidenhain soon added other facts pointing in the same direction. He found that if the blood supply be cut off from the submaxillary gland by compression of the artery the chorda still caused a secretion analogous to the post-mortem secretion after the heart ceases to beat.[21] Giannuzzi[18] discovered that by the injection of sodium carbonate or a dilute solution of hydrochloric acid into Wharton's duct a pronounced vaso-dilation ensued, on stimulation of the chorda, but no secretion. Heidenhain[23] found that quinine sulphate injected into the duct had a similar action, and that atropine[24] effectually paralyzed secretion, while leaving the vaso-dilator power of the nerve unaltered. Heidenhain[25] also discovered, and Langley confirmed his observation, that after the chorda tympani had been paralyzed by the action of nicotine, either injected subcutaneously or applied directly to the submaxillary ganglion, the chorda tympani recovered its secretory function before its dilator function. He observed, also, that after the chorda had been cut and allowed to degenerate for 2–3 days stimulation of the nerve still caused an increase in secretion, without an increase in the flow of blood from the gland's vein. This evidence showed that vaso-dilation might ensue without a secretion, that secretion might take place unaccompanied by vaso-dilation, and that secretion might be caused by stimulating dilator nerves after cutting off the blood supply. If these facts were true vaso-dilation could not be the cause of secretion, and hence that cause must be sought in some other gland element than the blood vessels.

Evidence of a more positive kind of the direct action of nerves upon the gland cells was not long lacking. Heidenhain showed that stimulation of secretory nerves caused well-marked changes in the structure of the gland cells.[21] He discovered that the specific constituents of the secretion were accumulated in the cell during glandular rest, and discharged from the cell during secretion. That these substances were not simply dissolved from the cells by the water stream passing through them he endeavored to show by the fact that on passing from a weak to a stronger stimulation of the chorda tympani, or other dilator secretory nerve, not only the rate, but also the concentration of the secretion increased. Apparently the more rapidly secreted saliva, although in contact with the cell contents for a briefer time, nevertheless dissolved more of them than that more slowly secreted. This obviously would have been impossible if the contents of the cell had not been rendered more soluble by the action of the nerve during the stronger stimulus. He brought forward, also, still more convincing evidence.[22] In the dog's parotid gland stimulation of the cervical sympathetic causes, generally, no secretion, but if this nerve be irritated coincident with the dilator secretory nerve the saliva secreted under the influence of both nerves is more concentrated than that secreted during irritation of the dilator nerve alone. Apparently the sympathetic, though causing no secretion, must, nevertheless, act on the cells, so as to render their contents more soluble. That this effect of the sympathetic could not be due to any possible action of the nerve on contractile tissue of the gland, as suggested by Schiff,[65] Eckhard[13] and others, Heidenhain believed von Wittich[77] had conclusively demonstrated. That the well-known high concentration of the sympathetic saliva could not be referred to the nerve's vaso-constrictor action Heidenhain[22] showed by the fact that, if the gland artery be almost totally compressed, the following chorda saliva was not rendered more concentrated.

These facts undoubtedly furnish strong evidence that the sympathetic and other nerves act on the gland cells, not only increasing the flow of water through them, but also rendering their contents more soluble.

Most of these facts, brought out chiefly in the salivary glands, have been found to be true for other glands. The independence of blood pressure and secretion, the inhibitory action of atropine, and an increase in concentration of the secretion coincident with a more rapid flow, have been observed by Afanassiew and Pawlow,[3] Gottlieb,[19] Pawlow and S.[58] Simonoskaja in the pancreas, stomach and other glands, in which secretion is normally accompanied by vaso-dilation. Sweat may be secreted during vaso-constriction or vaso-dilation, and in the cat's foot, twenty minutes after ligaturing the artery or cutting the leg from the body.[47] The skin glands of amphibia can secrete in the total absence of blood supply.[11] Moreover, of recent years, the importance of the *condition* of the secrcting cells, as a factor of secretion, has been clearly realized. The quick paralysis of some secretions during dyspnœa or by the action of drugs has emphasized this factor of secretion. Even in the kidney, where secretion apparently more nearly approaches a filtration, it has been shown that the condition of the capillary, or glomerular epithelium, and the character of the blood, exerts an influence on the secretion.[1] The possibility at once suggests itself that if the condition of the cells is so readily affected by external agents it may be modified by direct nerve action. The very rich nerve supply of many glands and the intimate association of nerve end and gland-cell undoubtedly bring strong confirmation to this supposition.

From this brief outline the extreme complexity of the problem of secretion will be manifest. Some secretions are accompanied by vaso-dilation; others by vaso-constriction. Some may persist twenty minutes after cutting off the blood supply; others are paralyzed within two or three minutes. Some are paralyzed by atropine and quinine; others are not. In the same gland stimulation of one nerve may cause the secretion of a large amount of watery secretion, while stimulation of another nerve causes the secretion of a small amount of exceedingly viscid secretion. There seems, in fact, to be no general rule of secretion true for all glands. The great difference between the phenomena of different secretions suggests that the mechanisms of

those secretions may be different in different cases. However probable it may seem, *a priori*, that there is everywhere one fundamental mechanism underlying all these secretions, a decent regard for truth forbids one accepting so far reaching a conclusion, unless it be supported by very strong evidence.

In the present paper, therefore, I wish to reopen the question whether all secretions are due to the activity of the gland cells, and to re-examine the evidence of the existence of nerves acting on those cells. The great theoretical and practical importance of Ludwig's conception is a sufficient excuse for a critical and experimental review, in the light of the physiology of the present day, of the evidence upon which that theory rests. Since the publication of Ludwig's and Heidenhain's work on secretion knowledge has been acquired of vaso-motor changes, osmosis, lymph formation as well as secretion proper, which might, possibly, cause even Heidenhain or Ludwig, if considering the subject at this time, to adopt a somewhat different interpretation of much of this evidence from that heretofore proposed. Such a review seems the more necessary for the reason that special applications of the theory have been, from time to time, questioned, and because, as will be apparent in the course of the following discussion, some of Heidenhain's inferences are unsound, owing to his having neglected to consider possibilities now known to be of importance. His recent extension of the theory to lymph formation, for example, has been seriously disputed by Starling,[68] Cohnheim and others. Starling especially has shown the uselessness of assuming any such secretory mechanism in certain special cases, and has thus thrown doubt upon the truth of the theory as a whole. Langley[37] has questioned the necessity for assuming distinct "trophic" fibres to explain salivary secretion, and for the kidney secretion special inferences of Heidenhain have been challenged by Senator, Adami[1] and v. Sobiranski.[67] The difference in pressure between blood and secretion observed by Ludwig may be readily accounted for on the basis of osmosis quite apart from any cell-activity.[28] The difference in temperature between saliva and blood has been denied by Bayliss and Hill,[5] working with bet-

ter methods. For some of the facts, also, errors of method greatly diminish the value of the testimony they offer, and some of that evidence depends upon the assumption that all secretions are probably due to the same cause. Hence, whether the theory of secretory nerves is true or not, it must be admitted, I believe, that little of the evidence which has hitherto been presented in support of that hypothesis can be accepted as it stands.

While fully aware, therefore, of the strong *a priori* probabllity that nerves may act on gland cells so as to affect osmosis through them, and while appreciating the strength of the evidence that they do so act, I feel myself compelled, for the reasons presented in the following criticisms of that evidence, to question whether secretion is really controlled in this manner.

But not only is the evidence upon which the secretory nerve theory rests inconclusive; there are also certain weaknesses in the theory itself which deserve more attention than they have hitherto received. It is by no means easy to understand how the nerve can affect the cell in such a way as to cause a secretion. The mere discharge of liquid from the cells into the gland lumen would, as pointed out elsewhere, lead to no secretion from the gland ducts. To obviate this difficulty Heidenhain supposed that, while the secretory nerve diminished the resistance of the inner end of the cell, the outer zone imbibed water from the lymph and capillary. The outer zone exerted an attractive pull upon the lymph. By the imbibition of this lymph the secretion was forced along the ducts. This explanation leads at once to difficulties. Not only is the explanation exceedingly hypothetical, but it is difficult to see why, if the pull on the lymph comes from the outer zone, secretion should be slowest after long stimulation, or during paralytic secretion, when the outer zone is at its greatest development, and how secretion can take place at all, or with any rapidity, in glands in which the outer zone has almost, or completely, disappeared, as in mucous salivary glands, the stomach or pancreas, after a long rest. It is also difficult to understand sympathetic secretion, which takes place during a period of vascular constriction. Nor can we ig-

nore the extreme complexity of the theory. The assumption that each, or any, cell of the sub-maxillary gland has acting upon it four totally different nerve ends is, in itself, highly improbable. A further difficulty is encountered when we critically examine Heidenhain's assumption that the trophic and secretory fibres are unequally distributed to the chorda tympani and sympathetic. It seems simple enough to refer the small secretion ensuing on sympathetic stimulation to the presence of a small number of secretory fibres in this nerve, but if it be asked whether these fibres innervate all the cells, or only a portion of them, we are at once plunged into a maze from which there is no way out. If they innervate all the cells we may ask why, if a few fibres suffice, more should be present in the chorda, and why the secretion should not be as copious as the chorda's. If they innervate a part of the cells only, new assumptions must be made to understand why stimulation of the sympathetic should exhaust the constituents of the whole gland. If we abandon the trophic fibres and postulate one sort of fibre only, the secretory, acting on the cell, Heidenhain's facts become largely inexplicable. Furthermore, when Heidenhain[26] assumed secretory nerves to the capillaries he undermined much of the evidence accumulated by him of secretory nerves to glands. For many of the facts of gland physiology might be understood by reference to these capillary nerves. Atropine, for instance, might conceivably prevent secretion by paralyzing the ends of the secretory nerves of the capillaries, thus inhibiting the production of lymph and fluid necessary for secretion.

In the present paper I have considered chiefly the physiology of secretion in the salivary glands. The experimental work has been devoted chiefly to studying the exceptional features of that secretion which have seemed difficult of comprehension on any other than the cellular theory of secretion. I have ventured, however, to bring some other secretions into relation with the conclusions concerning the mechanisms of salivary secretion.

It may prevent confusion and reconcile what might appear to be contradictory statements, to give here the chief conclusion drawn in the present paper. This is, that there is no single mechanism of secretion. In some glands the stored metabolic products are driven out of the cells by the action of muscle, as in Amphibian skin glands and sudoriferous glands; in others they are removed by currents of lymph, which are probably the result of osmosis, as in the pancreas, stomach, salivary glands; in some cases the cells imbibe water until they burst, and their contents rush into the gland-lumen, as in the intestinal cells of Ptychoptera larvæ; in others the inner end of the cell crumbles to pieces, as in the mammalian milk glands. Two, or more, of these mechanisms may coexist in one gland, and it is this which has rendered the physiology of such glands as the salivary so confusing. In the submaxillary gland, for example, I believe we have a muscular mechanism, innervated by the sympathetic; and an osmotic mechanism, innervated by the chorda. The sympathetic, in other words, causes secretion as Eckhard,[13] Schiff,* [65] and others[20] have maintained, by its action on contractile tissue in the gland body, thus mechanically compressing the ducts and alveoli and squeezing out the secretion. The chorda probably causes secretion, by its dilator action on the blood vessels. The following pages present the evidence for these conclusions.

Before proceeding farther it is necessary to define the sense in which the word "secretion" is here used. At present the word has no very definite significance, as it refers to different processes. For the sake of clearness it would be better to designate these various processes by different names. I suggest that, in the future, the word secretion be used to indicate the process of extruding subtances from cells into the lumen of the gland, the process of expulsion from the ducts, and the substances secreted by the gland. By this use of the word cellular secretion will be generally coincident in time with glandular. For the

* Schiff, loc. cit., p. 304, I. "It is probable that the great sympathetic which causes constriction of the parotid vessels causes, at the same time, the tissue of the gland to contract, and that by this contraction the gland empties itself of its contents formed independent of nerve action."

process of the formation of substance by the gland cell—a different process, but one at present included under secretion—I propose the name "Hylogenesis" (Gr. *ὕλη matter* and *γένεσις generation*), and for the substances formed the name "Hylogens." Thus trypinogen, mucinogen, pepsinogen are hylogens. The secretions consist of the hylogens plus water, salts and other substances derived unchanged from the blood. The present paper deals solely with secretion proper. Hylogenesis is considered elsewhere.* This word seems to me preferable to that of "Mesastates," suggested by Mr. J. N. Langley. Ranvier[62] and Van Gehuchten[73] wish to call the process here named hylogenesis, "secretion." This seems to me inadvisable, as thereby cellular secretion would correspond with glandular rest.

The experimental work embodied in this paper has been carried on chiefly in the Physiological Laboratory of Columbia University, and I am particularly indebted to Professor Curtis and Professor Lee both for extending to me facilities of the laboratories and for suggestive criticism. A portion of the work was done in the physiological laboratories of Cambridge University, England, and Marburg University, Germany. I desire to express my hearty appreciation of the courtesy of Professor Michael Foster and Professor Kossel in placing the facilities of their laboratories at my disposal. To Mr. J. N. Langley I am indebted for critical suggestions.

II. SYMPATHETIC SALIVARY SECRETION.

Stimulation of the upper end of the divided cervical sympathetic nerve of the cat, horse, dog, sheep or rabbit generally causes a secretion from the salivary glands. This secretion has everywhere† the same characteristic features, indicating that it is produced in all salivary glands in the same manner. These common features are the following: The saliva reaches its maximum rate of flow in the first 10 or 20 seconds, and then generally ceases, although stimulation lasts for several minutes. If sev-

* Shortly to appear in the *Journal of Morphology*.

† Except in the resting parotid and submaxillary glands of the dog. See next page.

eral stimulations follow closely, one upon the other, the amount of saliva secreted at each stimulation rapidly diminishes and often becomes nothing. Stimulation becomes then again effective if the gland be allowed to rest, if the chorda be irritated, or if liquid be injected into the gland duct. Finally, sympathetic secretion is invariably accompanied by vascular constriction, and the saliva, with the doubtful exception of that of the cat,[35] contains more organic matter than that secreted from the same gland under the influence of the dilator nerve.

That there are deviations from the typical course of a sympathetic secretion just sketched need hardly be said. Such deviations are probably due (see p. 309) to the changing fluidity of the saliva. When the saliva is thin, as in the horse, rabbit, cat or sheep, the secretion follows a very typical course; if the saliva be viscous, as in the resting salivary glands of the dog, the latent period is longer, and the secretion persists longer. These variations shed a not unimportant light on the mechanism of secretion.

To explain these typical phenomena, assuming the secretory activity of the gland cell, Heidenhain supposed that the sympathetic nerve carried three kinds of fibres: trophic, secretory and vaso-constrictor. The trophic fibres converted large quantities of mucinogen (submaxillary) into soluble mucin, making the juice rich in organic bodies; the secretory fibres caused secretion; the constrictor neutralized the secretory action and stopped secretion. The quick failure of the nerve on successive stimulations was referred to the exhaustion of nerve, nerve end, or gland cell.

The general features of sympathetic secretion seem to me, however, plainly to suggest that the secretion has been driven from the gland by a compression of the ducts and alveoli by some contractile tissue. I wish to consider these features separately, from this point of view, together with experiments bearing on their proper interpretation.

a. The Rate of Sympathetic Secretion.

Experiments I. and II.

Cat and dog. Submaxillary. Animals under ether. Canula in Wharton's duct, connected with a narrow tube graduated in millimeters, 250 mm. = 0.82 cc. Reading's every ten seconds

in mm. Chorda-lingual divided in each case. Cervical sympathetic divided and stimulated by tetanic shocks, secondary coil 180–100 mm. The chorda was first stimulated intermittently for an hour, so that the glands were secreting watery saliva.

	Cat. I	Cat. II	Cat. III	Dog. I	Dog. II
1st 10 seconds of sympathetic stimulation	10	9	10	25	17
2d " " " " "	9	5	6	4	2
3d " " " " "	0	0	0	3	2
4th " " " " "	0	0	0	2	1
5th " " " " "	0 off	0 off	0	2 off	2
6th " " " " "	0	0	0 off	8	1 off

By inspection of these figures, it is seen that on stimulation the secretion comes suddenly, reaches its maximum rate of flow in the first few seconds, and then quickly subsides. In the cat, it abruptly ceases after 20 seconds. In the dog, probably owing to the greater viscidity of the saliva and the resistance offered to its passage by the fine gland-tubules, it persists slightly throughout the stimulation.

Heidenhain attributes the abrupt cessation of secretion, after a few seconds, to the vaso-constrictor action of the nerve, in consequence of which the secretory mechanism is, as it were, suffocated.[23] That this explanation is incorrect may readily be shown by cutting off the blood by compressing the gland's artery, or by decapitation. In such cases, as the following experiments show, a perfectly typical secretion may ensue on stimulation of the sympathetic, ten or more minutes after ligaturing the artery, or decapitation.

Experiment Va.

(A full account of this experiment is given on page 343.)

Large dog, which had received 3cc. 1% morphine sulphate subcutaneously. Ether given through tracheal tube. Submaxillary dissected free, and remained attached only at the hilus and by its veins. Chorda-lingual and sympathetic cut. Canula connected with tube graduated in millimeters in Wharton's duct. Gland's artery exposed by extirpation of the digastric muscle. Tetanic shocks. Secondary coil at 150. The

secretion of the sympathetic is given in mm. at ten second intervals, 250 mm. = 0.82. cc.

Time.						Nerve Stimulated.	Secretion.
h	m	s	h	m	s		
3	25					The artery going to the gland was clamped close to the hilus.	
3	25		– 3	30		Chorda (intermittent)	Copious at first, it gradually ceases.
3	30					“	0
3	32					“	0
3	35					Sympathetic	**16, 3, 2, 2, 0, 0,** off.
3	37					“	0, 0, 0, 1, 0, 0, off.
3	40					“	0, 0, 0, 0, 0, 0, off.
						Interval (see page 317).	
3	42					Artery unclamped. Chorda stimulated intermittently for several minutes.	
4	07	30				Artery clamped.	
4	07	30	– 4	08		Chorda	155
4	08		– 4	09		“ (10 sec. int.)	30
4	09		– 4	11	30	“	16
4	12		– 4	13		“	0
4	13		– 4	14		Sympathetic.	**17, 4, 2, 2, 0,** off.
4	15		– 4	17		Chorda	0
4	17	30	– 4	18	15	Sympathetic.	**10, 4, 0, 0.**
4	20					“	0, 0, 0.
						Interval (see page 317).	
4	25					Sympathetic.	0, 0, 0.
4	26		– 4	27		Chorda.	0
						Interval (see page 317).	
4	29	30				Artery unclamped. The gland secretes spontaneously. Chorda stimulated intermittently.	
4	45	30				Artery clamped.	
4	46	30	– 4	47	30	Chorda	175
4	48	30	– 4	49	30	“	30
4	50		– 4	51		“	10
4	51	30	– 4	52	30	“	2
4	53						0
4	53		– 4	54		Sympathetic	**8, 2, 1, 0.**
4	54		– 4	55		Chorda	0, 0, 0, 0.
4	55	30	– 4	56	39	“	0
4	57		– 4	58		Sympathetic	**0, 4, 3, 0, 0.**
5	02					Artery unclamped.	
5	03		– 5	09		Spontaneous secretion.	
5	09		– 5	10		Sympathetic.	**9, 3, 2, 0, 0.**

Experiment V.

Large dog under morphine and chloroform. Right submaxillary gland prepared. Chorda lingual and sympathetic cut. Each nerve causes a good secretion. Readings as in previous experiments. Canula in Wharton's duct. Secondary coil 150. Tetanic shocks.

TIME. h. m. s.	h. m.	NERVE STIMULATED.	SECRETION.
5 49 30		Head cut off as rapidly as possible. Spinal cord and vertebral column not severed.	
5 50 30	– 5 55	Chorda (intermittent)	175
5 55		" (coil 70)	0
5 57		Sympathetic (coil 7)	**40, 20, 6, 2, 0.**
5 58	– 6 10	No stimulation.	
6 10		Sympathetic	**7, 5, 2, 0, 0.**

Experiment VI.

Dog. Conditions of experiment the same as in Experiment V. Submaxillary. Both nerves active.

TIME. h. m. s.	h. m.	NERVE.	SALIVA SECRETED IN MM.
4 30		Head completely severed from body.	
4 31 40	– 4 35	Chorda intermittent	65
4 35	– 4 38	Chorda.	0
4 38		Sympathetic.	**14, 3, 2, 2, 0.**

The foregoing experiments, demonstrating that a sympathetic secretion may be obtained ten minutes after all fluid and oxygen have been cut off from the gland shows, I think, that Heidenhain was wrong in ascribing the quick normal cessation of secretion during sympathetic stimulation to the nerve's action on the blood vessels. It is obvious that vascular constriction can have nothing to do with such cessation, because the changes produced in a normal gland by vascular constriction, namely, diminution of water and oxygen, have existed in all three experiments at least seven minutes before the nerve was stimulated, and continue during that stimulation without in any way affecting the course of the secretion.

Even a normal gland secreting a very viscous saliva furnishes evidence against the truth of Heidenhain's explanation. In the

resting submaxillary of the dog the sympathetic secretion may have a latent period of many seconds and persist for minutes. An instance of such a kind is the following :

Experiment III.

Large morphinized dog, receiving chloroform. Both chorda lingual and sympathetic cut. The submaxillary has not previously been secreting. Sympathetic stimulated by tetanic shocks. Secondary coil 15. Readings every 10 seconds in millimeters as before. *The saliva was extraordinarily viscid.* Total stimulation 2 minutes, 40 seconds. *Latent period 45 seconds.*

Amount of secretion : 0, 0, 0, 0, 5, 7, 7, 5, 5, 5, 4, 5, 5, 4, 4, 3 ; off, 3, 1, 0.

If secretion can begin after 42 seconds, and endure for two minutes, during a period of vascular constriction, as was the case in this experiment, it can hardly be assumed that vasoconstriction is the cause of the normal failure of that secretion within twenty seconds.

Heidenhain seems to have overlooked the fact that a sympathetic secretion may be obtained after cutting off the blood supply, at least five minutes after the chorda becomes inoperative. He referred the quick loss of the chorda's power in these experiments, to the suffocation of the gland cell.* If the loss of the chorda's secretory power is due to the paralysis of the gland cell by suffocation, the sympathetic must cause secretion in some other way than action on the cell, since this nerve causes a normal secretion long after the chorda has been paralyzed.

The quick gush of saliva and its abrupt cessation, as well as the anomalous cases represented by Experiment III, clearly indicate a muscular mechanism of secretion. They are probably to be explained as follows : On sympathetic stimulation the ducts

* Heidenhain, R. Hermann's Handbuch der Physiologie V, p. 46 : "Die Ursache der Verlangsamung der Absonderung bei hochgradiger Gefässverengerung oder Gefässverschluss liegt nicht in dem Sinken des Capillardruckes, sondern in der, mit der künstliche Anämie der Drüse verbundenen Verlangsamung des Blutstromes, bei welcher sich das Secretions Material, und namentlich der Sauerstoff für die Drüsenzellen allmälig erschöpft so dass der secretorische Apparat erstickt."

and alveoli are compressed and the liquid in them ejected. If that liquid is thin and runs readily, as in most albuminous glands, for example the parotid and submaxillary of the rabbit, sheep and horse, and the cat's submaxillary, or in mucous glands after long stimulation, the latent period is short, and the saliva is all expelled in from 10–20 seconds. Thereafter, although contraction persists, no more secretion escapes. If, on the other hand, the saliva is viscid, as in the first stimulation of a previously resting mucous gland (submaxillary and parotid of dog), it offers a great resistance in passing through the fine ducts and consequently requires a greater pressure and a longer time to start and to expel. Consequently the latent period is long and the secretion persists for some time. This explains the anomalous cases represented by Experiment III. In cases of very great viscidity, as in the parotid gland of the dog, the resistance may even be too great to be overcome by the compressing strength of the tissues. In this gland stimulation of the symyathetic either causes no secretion at all or very little, unless the saliva in the gland be previously diluted by the action of the dilator nerve. The muscular theory, too, readily explains why a typical sympathetic secretion can ensue in the total absence of blood supply.

b. The Decrease in the Amount of Saliva Obtainable upon Several Successive Stimulations.

If one sympathetic stimulation be followed by several others the amount of saliva obtainable on the second, or following stimulations, is much less than the first, and may be nothing at all.* If, however, the gland be allowed to rest, or if the chorda be stimulated, the nerve again produces a copious secretion upon sympathetic stimulation. This is shown in the following excerpts from experiments on the dog's and cat's submaxillary. Readings in mm. Stimulation in each case for thirty seconds. It is also clearly seen in Experiment VII, p. 311.

*This phenomenon has, of course, been often described. See among others Langley.[39]

	Cat. I. Amount.	Cat. II. Amount.	Dog.
1st stimulation	20	16	36
Rest	25 seconds	1 minute.	2 minutes.
2d stimulation	0	6	25
Rest	3 minutes	2 minutes	1 minute
3d stimulation	11	10.5	11
Rest			2 minutes
4th stimulation			10
Rest			1 minute
5th stimulation			11
Rest			1 minute
6th stimulation			2.5
Rest			Chorda stimulated
7th stimulation			25
Rest			2 minutes
8th stimulation			6
Rest			1 min. 40 sec.
9th stimulation			4

The great decrease in the amount of saliva obtainable on a second stimulation, closely following a first, even though a minute's intreval of rest elapse, might be explained on Heidenhain's theory, by assuming an exhaustion of secretory fibres, nerve ends or gland cells. Such an assumption is highly improbable. There is, I believe, no other example of a nerve end, or fibre, becoming exhausted by a weak stimulus of a minute's duration. That the secretory fibres of the chorda, their nerve ends and the gland cells are not exhausted or suffocated is shown by the fact that the following chorda stimulation is little, if at all, altered. The phenomena are clearly explicable, on the other hand, if the sympathetic causes secretion by compression of the ducts and alveoli. By the first stimulation the gland is largely emptied of its saliva. If no time be given for the ducts to be refilled, the following stimulation finds less available saliva, or none at all. The nerve appears, in fact, to have become inoperative until, through the resting of the gland, or the action of the chorda, the ducts be again filled. The exhausted element of the gland inferred by Heidenhain is the fluid in the ducts and alveoli.

c. The Augmentation of Sympathetic Saliva.

That the small amount of sympathetic secretion, in the cases just cited, is due to the presence of a small amount of fluid in the ducts and alveoli is indicated by the abnormally large sympathetic secretion, when the amount of liquid saliva in the gland is rendered abnormally large by stimulation of the chorda, or by the action of pilocarpine, nicotine and other drugs.

Langley[39] first observed the augmentation of sympathetic saliva by an immediately preceeding stimulation of the dilator nerve in the dog's parotid and submaxillary and the cat's submaxillary. The following experiments confirming Langley illustrates this augmentation.

Experiment VII.

Dog under morphine and chloroform, sympathetic and chorda cut. Canula in Wharton's duct. Secretion in mm. is given above the line for every 10 seconds, 250 mm. = 0.82 cc. Below the line is indicated the nerve stimulated; s, is the sympathetic; c, the chorda. If no letter is written, it indicates that at these intervals there was no stimulation.

10, 35, 31, 2, **25,** 4, 3, 2, 2, 8, 6, 4, **17,** 2, 2, 1, 2, 1, 3, 0
c s s s s s s s s s s s

3, 4, 1½, 1½, 1, 2, 1, 4, 4, 1, 1, 1, 1, 1, 2, 1½, 3, 1, 1, 0, 5, 1, 1, 0
s s s s s s s s s s s s s s s

½, 0, ½, ½, 8, 60, 38, 2, 1, **15,** 3, 1, 3, 3, 2, 2, 3, 2, 4, 1, 1, 2, 2, 1
s c c s s s s s s s

2, 1, 1, 2, 1, 45, 30, 3, **25,** 2, 4, 2, 3, 2, **3,** 1, 1, 1, 2, ———
s s s c s s s s s s s s c. c. c.

18, 7, 3, 3, 2, ½, 4, **2.** 2, ½, 1, ——————— **39, 29,** 4, 0, 0,
s s s s s s s s s Chorda 1 minute s s s s

4, 3, 2, 2, 1, 1, 3, etc.,
s s s s s

It will be noticed, in this experiment, that the first secretion of the sympathetic, immediately following the chorda stimulation, is abnormally large, but that the augmentation effect rapidly passes off. The augmented saliva, as Langley pointed out, is

more watery than normal and has a shorter latent period. It resembles chorda saliva. A similar watery and copious sympathetic saliva occurs after the injection of nicotine,[24] or pilocarpine,[22] and during paralytic secretion.[42]

This augmented saliva may be explained, assuming that the nerve acts on the gland cell, as follows: If the chorda and sympathetic act as the same gland cells (Heidenhain) it may be said that stimulation of the chorda renders the cells more responsive to a sympathetic stimulation immediately following. If, on the other hand, the chorda and sympathetic innervate different gland cells (Langley), we are forced to the assumption that nerve impulses traverse glands outside of the nerve tracts. "When either nerve is stimulated," Langley says, "there is an irradiation of impulses of less intensity to the cells in the neighborhood of those directly affected; that on stimulation of the chorda tympani the cells connected with it are left for a time in a state of weak excitation, so that irridiation of impulses reaching the gland by the sympathetic is much greater than normal, and these irradiating impulses being weak lead to a more fluid secretion."[39] It can hardly be said, I think, that either of these explanations is satisfactory. That irritability of the gland cells probably has nothing to do with this augmentation, but that it is the simple result of the presence of an abnormally large amount of fluid saliva in the gland is shown by the injection of innocuous fluid into Wharton's duct. By this means we passively distend the ducts and aveoli, without the intervention of cell activities. Following stimulation of the sympathetic causes an augmented secretion. I have tried such experiments only in the case of the dog's submaxillary, a somewhat unsatisfactory gland, owing to the viscidity of the saliva. The experiment, particularly if tried on a fresh gland full of viscous saliva, is not always successful. The cause of the failures has not been investigated, but I suppose they are due to the unavoidable driving into the gland of the viscous saliva and partly to the use of too great pressure in such cases, causing an over-distension of the ducts and a consequent injury to the nerves. The positive results are, however, sufficiently conclusive.

Experiment VIII.

Small dog under morphine and chloroform. Left submaxillary duct and nerves prepared. Nerves cut. The chorda is first stimulated intermittently for an hour. The sympathetic is stimulated each time for 30 seconds. Secondary coil 70. Secretion in mm. as before.

TIME. h. m. s.	NERVE	SECRETION.
3 30	Sympathetic	10
3 32	"	4
Inject ⅓ cc. 0.6% NaCl solution into Wharton's duct.		
3 34	Sympathetic	**15**
3 36	"	0
3 41	"	0
4 10	"	11
4 11	"	8
4 12 30	"	4
Inject ⅓ cc. 0.5% NaCl into duct.		
4 14	Sympathetic	**8**
4 15	"	6

Experiment IX.

Conditions of experiment as in 8. Dog larger. Sympathetic 30 seconds stimulation, unless otherwise indicated.

TIME. h. m. s.	NERVE.	SECRETION IN MM.
5 20	Sympathetic	40
5 22	"	15
5 24 –5 25	"	20
5 26	"	10
5 27 –5 27 40	"	20
5 28 –5 28 40	"	18
Inject .4 cc. 0.6% NaCl into duct.		
5 30	Sympathetic	**40**
5 31	"	7
5 32	"	0
Inject .3 cc. 0.6% NaCl.		
5 34	Sympathetic	**17**
5 35	"	2
5 36	"	0
Inject .3 cc. 0.6% NaCl.		
5 38	Sympathetic	**11**

The results of these experiments, in conjunction with those following, are most readily explicable, I believe, on the muscular theory. The augmented saliva, in whatever manner produced, gives fairly conclusive evidence that the nerve causes secretion by compression of the ducts and alveoli. If these are filled with an unusually large amount of fluid saliva an unusually large secretion, characterized by its short latent period and watery character, is secreted. If there be little saliva present, or if it be very viscous, we obtain a small secretion of long latent period and lasting for some time.

(*d*) Paralysis of the Sympathetic by Emptying the Ducts and its Restoral to Power by Injection of Fluid into the Ducts.

Further strong evidence of the muscular action of the sympathetic may be obtained by preventing the passage of fluid into the gland and stimulating the nerve until all available saliva in the ducts has presumably been expelled. The nerve then appears to have lost its action, but it may be shown to be still active by the injection of fluid into the ducts. The passage of fluid into the gland may be prevented either by the use of quinine or by compression of the gland artery.

Heidenhain * showed that if quinine sulphate be injected into Wharton's duct the secretory action of the chorda is ultimately paralyzed, but the gland becomes œdematous. This indicates that, although liquid is present in the lymph spaces, it is prevented in some way from passing through the cell. If, after paralysis of the chorda, the sympathetic be stimulated, a copious secretion is obtained. After a few stimulations, however, the nerve appears to be paralyzed. If that paralysis is only apparent, due to the emptiness of the gland's ducts, we should be able to obtain a secretion on sympathetic stimulation, by the injection into the duct of more quinine sulphate. The following experiment proves this to be the case.

* Heidenhain, Studien aus Breslau, IV, 1868.

Experiment X.

Large dog. Operation as in other experiments. Secretion in mm. 250 mm.=0.82 cc., s=sympathetic ; c=chorda.

Time.	Nerve.	Coil in cm.	Secretion in mm.
h. m. s.			
12 24	s	15	72
12 25	s	15	12

Chorda stimulated for several minutes, then .5 cc. of saturated solution of quinine sulphate injected slowly into Wharton's duct.

12 37	c	13	0
12 38	c	11	0
12 39	s	11	50
12 40	s	11	27
12 44	s	15	12
12 45	s	15	9
12 47	c	11	0
12 48	c	7	0
12 50	s	14	15
12 54	c	11	0
12 55	s	14	7
12 57	s	13	10
12 59	s	13	3

Inject mixture equal parts 0.6% NaCl and sat. quinine sulphate.

1 03	s	13	24
1 05	s	13	4
1 09	s	12	0
1 10	s	9	1
1 11	s	10	0

Neither nerve produces a secretion, though stimulated from time to time.

4 00	c	8	0
4 01	s	8	0

Inject 0.5 % NaCl into duct.

4 02	s	6.5	32
4 03	s	6	14
4 06	s	6	3
4 09	Inject HCl 0.5% into duct.		
4 10	s	6	9

Chorda ineffective at any strength.

In the foregoing experiment the chorda became completely ineffective at 12:30. The gland, however, was abnormally full of quinine fluid, and the first sympathetic stimulation after the

injection consequently gave a greatly augmented secretion at 12:39. Thereafter each stimulation yielded less and less, and finally at 12:59 only 3 mm. were secreted. The ducts may be assumed to be practically empty. Quinine solution was now again injected, and the next sympathetic stimulation yielded again a greatly augmented secretion. Finally at 1:11 the sympathetic failed to yield any secretion, and from then until 4 P. M. was totally ineffective. It would be said, at first sight, that the nerve was paralyzed. Such, however, was not the case, its seeming paralysis being due to the emptiness of the gland. This was shown by the injection of .5 % NaCl solution into the duct. The following stimulation of the sympathetic at 4:02 yielded a very large secretion.

This experiment in two ways furnishes very strong evidence of the muscular nature of the sympathetic secretion. The fact that sympathetic secretion may be obtained long after paralysis of the chorda is very suggestive. Heidenhain* maintains that the chorda secretion is paralyzed by the action of the drug on the gland cells. If this be true, and I see no reason to doubt it, it furnishes very strong evidence that the sympathetic produces its secretion in *some other manner* than action on the gland cell, for the sympathetic secretion is not materially affected long after the gland cells have been completely paralyzed. The fact that the nerve's effect soon passes away, but may be restored by the simple injection of more quinine solution or other fluid into the duct, I believe to be susceptible of but one explanation, *i. e.*, that the nerve causes this secretion by compression of the ducts and alveoli.

A similar phenomenon is witnessed if the gland artery be compressed and fluid thus cut off from the gland. A few stimulations of the sympathetic suffice to render the nerve inoperative, but by injection of fluid into the duct the nerve is shown to be still active.

* Heidenhain, Studien aus Breslau, IV, 1868, p. 85, "so wird die Erregbarkeit der absondernden Elemente bald herabgesetzt und nach kurzer Zeit ganz vernichtet."

Experiment Va (Continued; see p. 305).

TIME. h. m. s.	NERVE.	SECRETION IN MM.
3 25	Artery clamped close by the hilus.	
3 30	Chorda	0
3 35	Sympathetic	23
3 37	Sympathetic	0
3 40	Sympathetic	0
	0.2 cc., .5 % NaCl solution injected into duct.	
3 41	Sympathetic	17
3 42	Artery unclamped	
4 07 30	Artery clamped	
4 12	Chorda	0
4 13	Sympathetic	25
4 15–4 17	Chorda	0
4 17 30–4 18 15	Sympathetic	14
4 20	Sympathetic	0
4 23	.3 cc., .5 % NaCl injected into duct	
4 24	Sympathetic	13
4 25	Sympathetic	0
4 26–4 27	Sympathetic	0
4 28	.2 cc., .5 % NaCl injected	
4 29	Sympathetic	8

In this experiment the sympathetic appeared paralyzed at 3:40, 4:20 and 4:26, but the injection of normal salt solution into the duct was followed by a secretion little less than normal, on the next stimulation. In one case twenty minutes after the artery had been clamped, the sympathetic was thus shown still to be active. Heidenhain attributes the loss of the chorda's power to the suffocation and consequent paralysis of the gland cell. (See footnote, p. 308.) As already pointed out (p. 316) this would, if true, show that the sympathetic produces its secretion in some other way than by action on the cell. The fact that the nerve's power may be restored by the injection of innocuous fluid into the ducts is readily explicable on the muscular theory of secretion, but, with difficulty, on the cellular theory.

I found that a similar phenomenon may, at times, be seen in the cat's submaxillary, which has been paralyzed by just sufficient atropin to prevent chorda secretion. As was first pointed out by Langley, atropin paralyzes the sympathetic in the cat, but more atropin is required than to paralyze the chorda. The

sympathetic may appear paralyzed, wholly or in part, before it actually is. In this condition gently forcing the secreted saliva back into the gland restores the nerve's power.

Experiment XII.

Cat etherized. Canula in duct of left submaxillary. Both chorda and cervical sympathetic cut. Both nerves active. Inject .1% solution of atropin carefully into femoral vein until chorda just paralyzed. Sympathetic stimulated 30 seconds each time.

TIME. h. m. s.	NERVE.	SECRETION IN CC.
3 50	Chorda	0.
3 51	Sympathetic	0. 1
3 52	"	0. 1
3 53	"	0. 1
3 54	"	0.05
3 55	"	0.05
3 56	"	0.03
	Blew the secretion gently back into gland.	
3 57	Sympathetic	0.13
4 00	"	0.15
4 06	Inject .1 cc. atropin into femoral vein.	
4 07	Sympathetic	0.10
4 08	"	0.10
4 09	"	0.10
4 10	Sympathetic	.10
	Inject .2 cc. atropin	
4 13	Sympathetic	.07
4 14	"	.01
4 15	"	.03
	Blew saliva into gland.	
4 16	Sympathetic	.25
4 17	"	.05
4 18	"	.04
4 19	"	.02
	Blew .1 cc. saliva back into gland.	
4 20	Sympathetic	.12
4 21	"	.04
4 22	"	.03
	Blew .1 cc. saliva back into gland.	

4 23	Sympathetic	**.14**
4 24	"	.02
4 25	"	.04
4 26	"	.02
	Blew .1 cc. saliva back into gland.	
4 27	Sympathetic	**.13**
4 28	"	.01
4 29	"	.06
4 30	"	.02
4 31	"	.03
	Blew .1 cc. saliva back into gland.	
4 32	Sympathetic	**.10**
4 33	"	.02
4 34	"	.05
4 35	"	.04
4 26	"	.03
	Blew back .1 cc. saliva.	
4 37	Sympathetic	**.12**
4 38	"	.04
4 39	"	.01
4 40	"	.04
4 41	"	.03
	Blew back .1 cc. saliva.	
4 42	Sympathetic	**.09**
4 43	"	.03
4 44	"	.04
4 45	"	.02
	Blew back .1 cc. saliva.	
4 46	Sympathetic	**.10**
4 47	"	.05
4 48	"	.03
4 49	"	.04
4 50	"	.02
	Blew back .1 cc. saliva.	
4 51	Sympathetic.	**.11**
4 52	"	.02
4 53	"	.04
4 54	"	.025
	Blew back .1 cc.	
4 55	S	.075
4 56	S	.025
4 57	S	.04 &c.

The most probable explanation of the apparent failure, partial or total of the sympathetic, in all the immediately preceding experiments, appears to me to be this: That by the injection of

quinine, or atropin, or compression of the gland's artery, liquid is prevented from entering the gland. A few stimulations of the sympathetic suffice to expell all, or most, of the available saliva in the gland, and the nerve thereafter appears paralyzed. If, now, the ducts and alveoli be passively redistended by the injection of liquid into the duct the nerve again causes a compression of the duct, and the fluid is again expelled and gives a secretion. This renewed secretion cannot, however, be referred to the action of the gland cell, because the latter has been in one case paralyzed by the action of quinine, and in the other case by suffocation. Nor could it be referred to the action of the cell, even were the latter not paralyzed, for the mere hypothetical taking-up of fluid into the cell from the duct, and its discharge again into the latter, would in no way alter the bulk of fluid in the ducts plus the bulk of the cell. There would, hence, be no pressure to drive the secretion from the gland.

e. The Character of Sympathetic Saliva.

Evidence that the sympathetic nerve innervates the gland cell has been derived from the character of the sympathetic saliva. This, as is well known, is richer in organic matters than the saliva secreted under the influence of the gland's dilator nerve. This greater richness Heidenhain attributes to the predominance in this nerve of so-called "trophic" fibres, the function of which is to render the stored-up metabolic products of the cell (hylogens) more soluble, and the juice consequently more concentrated. This assumption involves such consequences that by common consent it has been considered the most unsatisfactory part of the Heidenhain theory. It is, however, practically the only probable explanation, with one exception, which has been offered. The exception is the view suggested by Schiff, discussed below.

If the sympathetic simply drives out the saliva already present in the gland the sympathetic saliva must be of the character of that present in the ducts and alveoli at the moment of stimulation. There is evidence that this is the case. That the saliva in the ducts of the dog's parotid is very viscid has been shown by

Langley.[39] Sections show the ducts plugged with a viscous looking mass, and Langley suggests that the saliva is here too thick to be expelled. In one experiment Langley found a dog's parotid which secreted under the influence of the sympathetic 1.3 cc. Concerning this saliva Langley says:[40]

"The saliva was of the most remarkable nature; it formed a thick jelly-like mass; if allowed to collect at all in the canula it could be drawn out as a continuous clot. During the experiment the duct was frequently emptied by pressure to prevent its being stopped up." The saliva contained 7.8 % of organic solids. We can, moreover, artificially alter the fluidity of the saliva in the ducts, rendering it more dilute, by the action of the chorda tympani or pilocarpine. In such cases, as we have seen in speaking of the augmented secretion, sympathetic saliva is almost as thin as chorda saliva. By long stimulation of the chorda, moreover, we may exhaust the soluble constituents of the gland. In such cases it may be presumed that the gland saliva is thinner than normal. It is known that under such circumstances the sympathetic saliva may fall within the limits of density of chorda saliva.* A similar change occurs in paralytic secretions following division of the chorda. The gland then secretes a very thin saliva, and sections show the cells practically exhausted of their mucous. The sympathetic in these causes a very abundant and very watery secretion.

We may obtain still further evidence of the character of the saliva normally present in the ducts of the resting gland by a sudden, strong stimulation of the chorda tympani. The rapid inflow of fluid from the capillaries about the alveoli, taking place under the influence of that nerve, drives out the saliva in the ducts before it has time to become diluted. If we examine this saliva first appearing on chorda stimulation we find it in all respects typical sympathetic saliva. From this Schiff concluded† that sympathetic saliva was nothing more than the saliva normally present in the ducts, formed during glandular rest.

* Heidenhain, Studien aus Breslau, IV, 1868. After long sympathetic stimulations the saliva becomes "dünnflussig, hell, und dadurch dem chorda Speichel ganz und gar ähnlich."

† Schiff. Leçons sur la Digestion. Tome I., p. 296, 1867; also p. 304.

Schiff found that if the sympathetic nerve of the horse be stimulated the parotid secreted quickly 8–10 volumes of white saliva, and then, as in the cat's submaxillary, secretion ceased. If the horse be fed there ensued a copious, clear secretion of watery cerebral saliva. The gland was now, presumably, full of such saliva. If it be allowed to rest for twenty minutes without secretion on again feeding the horse the first saliva (8–10 volumes) *was typical, thick, white sympathetic saliva.* This was followed by the clear cerebral saliva. Schiff repeated this many times, thus showing that in the interval of rest the gland, uninfluenced by the sympathetic, converts the clear cerebral saliva into typical so-called sympathetic saliva. A similar phenomenon has been described, with a somewhat different interpretation for the dog's submaxillary, by Heidenhain.* I have repeated Schiff's experiment on the dog's submaxillary, fully confirming him. This is shown in the following experiment.

Experiment XIII.

Large dog, morphine and ether. At 10:30 A. M. canula in right Wharton's duct. Sympathetic and chorda-lingual cut. On the first stimulation of the chorda the first saliva was viscid, whitish and filled with corpuscles. The chorda was stimulated until 2 cc. of saliva were secreted. This saliva was thin, clear, typical chorda saliva. Gland rested without secretion until 11:30. Stimulated chorda. *The first saliva was thick, viscid, white saliva.* The gland then secreted 1 cc., clear chorda saliva. Rested until 2:30 P. M. Stimulated the chorda. *A very large amount of typical, sympathetic saliva* appeared first, followed by 2 cc. of watery chorda saliva. Gland rested until 4 P. M. Stimulated chorda. *The first saliva was viscid and contained many salivary corpuscles.* Secreted afterward 1 cc. clear saliva. Rested until 5 P. M. Stimulated the chorda. *The first saliva was again viscid, whitish saliva, filled with salivary corpuscles and lumps.*

* Heidenhain. Studien aus Breslau, IV, 1868, p. 52. "Die erste Speichel portion war sehr dick, fast gallertartig, reich an Schleimballen wie sie sonst im Sympathicus Speichel vorkommen, und ebenso an Speichelkörperchen die haufenweise bei einander lagen."

This experiment proves that after each stimulation of the chorda, the thin, chorda saliva filling the gland ducts is quickly converted, even in the absence of sympathetic influence, into typical viscid, sympathetic saliva.* It shows, also, that the ducts of the normal, resting mucous gland are filled with saliva, supposed to be characteristic of the sympathetic's action. This observation seems to me to render Heidenhain's assumption of special "trophic" nerve fibres to account for the character of such saliva, superfluous; and, also, to give additional evidence that sympathetic saliva is nothing more than this "saliva of rest," expelled by compression of ducts and alveoli. The correctness of the latter view is, in my opinion, strongly confirmed by the great variation in character of sympathetic saliva, with a variation of character of the saliva within the gland.

I wish to point out, also, that the influence of sympathetic stimulation upon the composition of the saliva secreted during coincident stimulation of the dilator nerve, upon which special stress has been laid by Heidenhain, is also readily understood on this hypothesis of the nature of sympathetic action. Langley's discovery[39] that the sympathetic produces a secretion from the dog's parotid unless the saliva be too thick for expulsion make Heidenhain's results clear.[22]

Heidenhain found, in harmony with all other observers, that stimulation of the sympathetic usually causes no secretion from the dog's parotid. He concluded from this that the nerve carried no, or few, secretory fibres.† He discovered, however, that if Jacobson's nerve be irritated so as to cause a secretion, and during this irritation the sympathetic be stimulated, the saliva secreted during simultaneous irritation of both nerves was far richer in organic solids than that secreted under the influence of Jacobson's nerve alone.‡ Denying that the sympathetic

* This is a pretty conclusive reply to the statement of Heidenhain that the simple contact of the water with the hylogens is not sufficient to dissolve them
We have here a demonstration that it is sufficient in the total absence of nerve influence.

† Heidenhain. Hermann's Handbuch d. Phys. V, p. 55. "Der Sympathicus des Hundes enthält für die Parotis nur trophische, für die submaxillaris daneben wenige secretorische Fasern."

‡ Heidenhain, Hermann's Handbuch d. Phys. V, p. 55.

exerted a secretory effect upon the gland, he considered the secretion to be due to Jacobson's nerve alone. He concluded, therefore, that stimulation of the sympathetic enormously increased the content of organic solids in the cerebral saliva. The sympathetic must hence act on the gland cells so as to render their contents far more soluble. From Langley's results, however, we can safely conclude that the saliva, secreted when both nerves are stimulated, is not pure cerebral saliva, but largely, if not wholly, augmented sympathetic saliva. Like all sympathetic saliva, it is more concentrated than the saliva secreted under the influence of the dilator nerve, because it is expelled without dilution.

f. Other Evidence of the Muscular Nature of the Mechanism of Sympathetic Secretion.

Very clear evidence, also, has been brought forward by Eckhard,[13] von Wittich[77] and Heidenhain[21] himself that the sympathetic causes at least the major part of its secretion, by a compression of the ducts and alveoli. The parotid gland of the sheep is an albuminous gland, capable of secreting against a pressure of 400–500 m. m. of water (Eckhard). If while secreting against a somewhat lower pressure (200–300 mm.) the cervical sympathetic be stimulated, the water rises suddenly in the manometer for some distance (30–100 mm.). *On ceasing stimulation the secretion rushes back at once into the gland nearly, though never quite, to its former level.* The higher the pressure the more sudden the flow backward. The quick rise at the beginning of stimulation and the abrupt back flow of the secretion at the end plainly suggest that the nerve caused compression of the ducts and alveoli, and thus pressed out the secretion. On ceasing stimulation these structures dilated, and the secretion, being under pressure, rushed back into the gland. I see no other explanation for the back flow, as it takes place too suddenly and at too low a pressure (200 mm. water) to be due to back filtration.

Heidenhain's observation is less striking, but it is similar to

the above. (Breslau Studien, p. 69, IV.) In taking the secretory pressure of the dog's submaxillary he stimulated the chorda until the pressure in the ducts was 271 mm. Hg. On ceasing stimulation the manometer gradually fell. *On stimulating the sympathetic the sinking became much slower*, and the manometer remained stationary at 160 mm. On breaking the stimulation the manometer sank gradually to 100. On stimulating the sympathetic it rose to 107, and on chorda stimulation to 271. It gradually fell during following sympathetic stimulation, *but on breaking the stimulation it fell with striking rapidity* (Auftälig beschleunigtes Sinken). Heidenhain thus records for the dog's submaxillary the same sudden back flow on breaking the stimulation of the sympathetic as Eckhard and von Wittich describe in the sheep.

Paradoxical though it may seem, the experiments just quoted of von Wittich and Eckhard have been cited by Heidenhain as conclusive evidence that the sympathetic does not simply drive out the secretion already in the gland. And it is this conviction which led Heidenhain, in the discussion of all experiments involving the sympathetic, to ignore the possibility of its having such an action. Heidenhain believed von Wittich was right in contending that the failure of the manometer to return to its former level on breaking stimulation proved that the amount of saliva in the gland had been increased. It will be instructive to consider von Wittich's explanation of the phenomena of this secretion. von Wittich[77] suggests that the back flow of the saliva is due to the saliva being pushed back into the cells. Let us examine this more closely. von Wittich and Heidenhain assumed that the cells, on stimulation, discharge their stored products into the lumen. Such a process, it need hardly be said, would lead to no secretion from the ducts, as the bulk of the cell would diminish to just the extent that the bulk of fluid in the ducts increases. Hence the bulk of cell plus liquid would remain unaltered. We must, therefore, make either one of two farther assumptions: First, that the alveoli are greatly distended owing to the turgor of the cells. Stimulation of the nerve might conceivably diminish the resisting power of

the inner end of the cell, and the secretion be expelled from the cell by intra-cellular tension, and from the ducts by the elastic tension of the distended alveolar wall. Or, second, it must be assumed that, as the fluid flows from the cell, new fluid enters the cell from the rear, so that the cell does not diminish in bulk to an extent aqual to the bulk of secretion it has lost. Either of these assumptions lands us at once in difficulties. If the first be true we cannot understand why the sympathetic secretion should be abnormally large, just in those cases, such as paralytic secretions, or after long-continued chorda secretion, in which the alveoli are not distended and are not presumably under pressure. The second assumption, besides being wholly imaginary, has to explain whence comes the fluid flowing into the cell, and why it should flow in during sympathetic stimulation at a time when there is a pronounced vaso-constriction.

With this difficulty of understanding how the nerve could cause a secretion by action on the cell, let us see how the sudden back flow could be understood. According to von Wittich and Heidenhain the diameter of the alveoli has remained constant. The secretion, manifestly, cannot upon this assumption return into the gland, unless there be a diminution in the combined bulk of the secretion in the ducts and the cells. There will be no such alteration in bulk, however, by the secretion passing into the cell as von Wittich assumes, for the cell will grow to just the amount that the secretion in the lumen diminishes. The only way a diminution in bulk could be brought about is by a back filtration. The fall is, however, much too sudden for this, and takes place at a pressure much less than the gland can sustain without becoming œdematous. It is also impossible to see why on ceasing stimulation the permeability of the gland to back filtration should suddenly increase. Easy though it seems at first sight, therefore, to ascribe such a back flow to a reabsorption under pressure of saliva by the cell, closer inquiry shows that it is impossible to account for this back flow except on the assumption either of a back filtration or that there has been an alteration in the diameter of the alveoli. I maintain with Eckhard that a back filtration is highly improbable, and there re-

mains only the alternative of an increase in the diameter of the alveoli, probably following an active compression.

But if the saliva is simply pressed out, why is it that it does not return to its former level on ceasing stimulation? This was supposed by von Wittich to prove that the nerve increased the amount of saliva in the gland. I fully agree with von Wittich in this contention, but I disagree with him entirely in referring the increase to the action of the nerve on the cell This increase may be readily understood on the muscular theory, without any assumption of nerve activity on the gland cell, as follows: On breaking sympathetic stimulation of considerable duration a temporary vaso-dilation occurs and the ducts and alveoli relax. It takes an appreciable time for the saliva to pass back into the fine tubules, and during this time the cells are absorbing water from the lymph and capillaries. Hence their bulk and the amount of saliva is increased and the saliva is never able to return to its former level. The proof of this is sufficiently clear. That vaso-dilation does occur temporarily on ceasing stimulation of constrictor nerves has often been remarked. I have myself often seen it in the rabbit's ear and in the cat's submaxillary. In the dog's submaxillary I have often seen, also, that coincident with this vaso-dilation a slight secretion may actually ensue (See Expt. VII, p. 311). It is, also, well established that the cells do imbibe fluid and food during or after sympathetic stimulation and thus increase the bulk of undifferentiated protoplasm.

In view of these facts, I believe that von Wittich's and Eckhard's experiments, instead of proving that sympathetic stimution can not possibly be due to compression of the ducts and alveoli, demonstrates that it must be due to such compression; that it is impossible to account for the back flow on any other probable hypothesis, and that the fact that the saliva does not reach its former level is readily understood by reference to the nerve's constrictor action and the temporary vaso-dilation ensuing on breaking simulation. I do not believe that von Wittich ever endeavored to analyze in detail his own explanation, or he must have perceived its impossibility.

g. The Location and Nature of the Contractile Substance in the Gland.

The contractile tissue, responsible for the sympathetic secretion, resides neither in the gland capsule nor in the capillaries. Glands dissected free from the capsules secrete normally. The capillaries cannot be held responsible, as Vierheller [71] supposed, because, as one may readily see in the cat's submaxillary, the nerve may be still active on the blood vessels while producing no secretion, and von Wittich[78] records that after curare, the rabbit's sympathetic loses its secretory activity while still active on the blood vessels of the ear. Unna[70] has suggested that the basement membrane is contractile, and this may possibly be the case. There is, however, no evidence of it. That there is smooth muscle about some of the principal ducts of the salivary glands is well-known, but most histologists have failed to find any between or about the alveoli. However, Pflüger [60] and Schlüter[66] have each described isolated fibres, and strands of smooth muscle lying between the alveoli, distinct from the blood vessels, "so that the stroma is not entirely lacking in contractility."

Whether the contractile tissues thus far recognized histologically in the gland are those active in the production of this secretion appears to be doubtful. The physiological evidence is of itself so strong, however, that I believe we can safely assume the existence of such a tissue, even had we no histological evidence of its presence.

h. The Changes in Gland Cells upon Sympathetic Stimulation.

The changes in gland cells, induced by stimulation of the sympathetic nerve, are most clearly seen in the rabbit's parotid,[40] less clearly in the dog's parotid, where the nerve causes normally little or no secretion. The changes consist in the diminution in the size of the cell, the discharge of the mucous or secretory products, the formation of new undifferentiated protoplasm and

in the nucleus becoming round and moving toward the center of the cell. These changes are identical in kind with, though taking place generally more slowly than, those following stimulation of the dilator nerve or the injection of pilocarpine. Do they indicate the direct action of the nerve on the cell? Although they might be so interpreted, they may be readily understood without any such assumption, as follows: Stimulation of the nerve causes a compression of the cells and thus expels from them their stored-up metabolic products and liquid. By this means the cells discharge their products. On ceasing stimulation the alveoli and ducts relax, and the cells take up water and food from the lymph. The latter process is hastened probably by a temporary vaso-dilation ensuing when the sympathetic stimulation is broken. In virtue of the food, oxygen and lymph thus brought to them the cells form new undifferentiated protoplasm. On several successive stimulations the accumulated metabolic products are largely discharged, the cells become smaller and the nuclei, relieved from pressure, become round and move toward the center of the cells. The same explanation holds also for the changes following stimulation of the dilator secretory nerve, with the exception that the stored products are dissolved out of the cell, instead of being squeezed out, and as vaso-dilation accompanies this secretion the changes take place at a more rapid rate. These changes are discussed more at length in my paper on the Pancreas Cell.*

i. Summary and Conclusion.

The phenomena of sympathetic secretion, which have been considered, could hardly indicate more clearly, I think, the muscular mechanism of that secretion. The sudden gush of saliva; its sudden cessation, however prolonged the stimulation; the diminution in the amount of saliva secreted when the stimulations are rapidly repeated; the apparent paralysis of the nerve when the ducts are empty and its restoral to power if the ducts be passively redistended; the augmentation in volume of the secretion, when the ducts are abnormally full of fluid saliva, and the

* Shortly to appear in the *Journal of Morphology*.

diminution in amount of secretion when there is little saliva present; the dependence of the character of the sympathetic saliva upon that present in the gland at the moment of stimulation; the back flow of saliva into the gland on stopping stimulation when the gland is secreting against pressure; the presence of smooth muscle in the ducts and between the alveoli—these facts point unmistakably in one direction. A stronger chain of circumstantial and direct evidence that this secretion is caused by compression of the ducts and alveoli by contractile tissue would be hard to imagine. If some of these phenomena are susceptible of explanation upon the hypothesis that the secretion is due to gland cell activity, others of them, *i. e.*, the augmented salivary secretion, the back flow of saliva on breaking stimulation, the paralysis of the nerve when the ducts are empty, and its restoral to power if the ducts be redistended, are explicable, if at all, by that theory, only by means of improbable and unproven assumptions.

The surprisingly ready acceptance of the Ludwig-Heidenhain theory of secretory nerves, acting on gland cells, as an explanation of the sympathetic salivary secretion in the face of unmistakable indications of a muscular mechanism, has been due, largely, I believe, to the generally prevalent belief that there is but one mechanism of secretion. That this belief is erroneous, there has long been, I believe, many indications. For there is direct evidence in many glands, such as the poison glands of snakes, the skin glands of amphibia, many unicellular glands, sebaceous and sweat glands, that many secretions are due to muscular action. And in many other glands the phenomena of secretion have shown as clearly that here the mechanism was some other than muscular. There must evidently be at least two different mechanisms, a muscular and some other one. Once the idea that there is but one mechanism of secretion is abandoned, the salivary secretions will be found, I believe, to lose much of their puzzling character.

The facts which Heidenhain urges as showing that the sympathetic produces secretion by action on the gland cell are readily accounted for if the sympathetic cause compression of the ducts and alveoli and vaso-constriction.

III. OTHER SECRETIONS DUE TO MUSCLE ACTION.

Probably many other secretions are due to muscle action.

The unicellular glands of the carp-louse, Argulus foliaceus, are surrounded by muscle fibres. Nussbaum,[55] observing the living glands, states that they are emptied by the contraction of this musculature. Muscle surrounds the unicellular glands in the mantel of Aplysia,[8] and the glandular pedicellaria of the Echinoderms.[34] The gasteropod liver[4] possesses, beneath the serosa, an incomplete musculature, the contraction of which has been watched in the living gland. A similar sheath is found in the livers of Crustacea, land and water Isopods, Amphipods and Decapods.[74]

The poison glands of spiders have their alveoli enclosed in a tunic of spirally arranged muscular fibres.[51] In the salivary glands of Cephalopods[63] the cells rest on connective tissue, which is, in turn, surrounded by muscle fibres. An examination of the physiology of these glands leaves little doubt that the secretion is due to muscular action.[31] The amphibian skin glands are surrounded by a muscular sheath lying between the cells and the basement membrane. There is no doubt from observations on the living glands (Engelmann,[16] Drasch,[11] Ranvier[62]) that this muscle at times contracts, compresses the gland and thus causes a secretion. A similar muscular mechanism prevails in the mucous glands of Petromyzon, in which the cells are bodily extruded.

The poison glands of amphibia and reptilia and others of the salivary glands[76] are provided with their own musculature, or are emptied by surrounding skeletal muscles. Many anal and cloacal glands,[45] sweat[62] and sebaceous glands are provided with a musculature lying between the basement membrane and the cells. There is little doubt that the secretion of sebum is produced by the action of this muscle. The same can be said for the secretion of the oil gland of birds. Probably the most interesting secretion due to muscular action, outside of the salivary glands, is found in the mammalian sweat glands. From

the observations of Ranvier,[62] Joseph[29] and others certain secretions of sweat are probably due to the compression of the gland by this muscle. Probably the post-mortem sweat secretions, secretion after closing the artery, or the injection of strychnia are due to this cause. (There is, however, a second sweat mechanism associated with vaso-dilation.)

Many more examples of the muscular mechanism of secretion might be given, but these suffice to indicate the very wide distribution of such a mechanism. Muscular mechanisms are, possibly, more common among the invertebrates, but they play, also, a not inconsiderable part in vertebrate secretions. The vertebrate, however, with its delicately coördinated, closed vascular system, develops a second mechanism, that of osmosis, which we will now consider.

IV. SALIVARY SECRETION ENSUING UPON STIMULATION OF THE VASO-DILATOR NERVE.

That the general features of chorda secretion coincide with the phenomena of osmosis, regulated by the nerve's dilator action, is pointed out briefly on p. 356. I wish here to consider more particularly those facts which have hitherto been irreconcilable with such a theory, and have been generally considered evidence of a special action of the nerve on the gland cell. These facts are the most important evidences of a secretory nerves and so warrant a careful consideration. They are: (*a*) the increase in the percentage of organic solids of a secretion coincident with an increased rate of secretion; (*b*) the action of atropine; (*c*) the chorda-secretion after clamping the artery; (*d*) the action of nicotine.

a. The Increase in the Percentage of Organic Constituents coincident with an Increased Rate of Secretion.

Heidenhain * observed that on passing from a weak to a strong stimulation of the dilator nerve in the fresh submaxillary and

* Heidenhain. Hermann's Handbuch der Physiologie V. p. 50. Studien aus Breslan IV, 1868, p. 32.

parotid gland of the dog, not only was the rate of secretion increased, but also the percentage of solids. He obtained a similar result in the dog's pancreas, Gottlieb[19] in the rabbit's pancreas, and Pawlow and Schumowa-Simanowskaja[58] in the dog's stomach. In the sheep's submaxillary, on the other hand, there was little or no increase in the per cent. of solids on increasing the stimulus.

No. of Stimulation.	Time.	Coil.	Am't. of Secretion.	Rate of Secretion in 1 min.	Solids.	Salts.	Organic Constituents.
	h. m. m.						
1	9 20–45	315—288	3.5	0.14	0.74	0.22	0.52
2	9 47–51	160—130	3.5	.87	2.10	.56	1.54
3	10 54.5–59	100— 60	3.0	.66	2.08	.45	1.63
4	10 19–40	264—245	2.8	.11	1.44	.36	1.07
5	10 45–48	160—130	3.0	1.00	1.41	.49	0.91
6	10 50–56	80— 65	3.0	.50	1.16	.39	0.76
7	11 9–27	270—250	2.5	.13	0.78	.30	0.48
8	11 30–34	150—120	3.1	.77	0.90	.38	0.51
9	11 35–44	80— 30	2.8	31	0.79	.36	0.42

Heidenhain believed that this increase in solids meant that the cerebal nerve, besides quickening the flow of water through the cells, rendered the cell contents more soluble. How otherwise shall we explain the fact, he asks, that although given a shorter time of contact with these solids, the water passing through the cells, nevertheless dissolves more than during slow secretion. "Die blosse Berührung mit der aus dem Blute ausgeschiedenen Flüssigkeit ist zur Überfuhrung des Schleimes in das Secret nicht ausreichend, denn sonst musste das Secret um so reicher daran sein, je länger die Flüssigkeit in den Drüsenräumen verweilt, d. h. je langsamer die Secretion vor sich geht."[21] He further assumes that the trophic fibers require a stronger stimulus than the secretory. "Das cerebrale Secret wird, so lange die Drüse unermüdet ist, bei Reizverstärkung reicher an organischen Bestandtheilen, weil der Umsatz der organischen Substanzen in den Zellen unter den Einflusse der stärker gereizten trophischen Fasern schneller steigt, als der Wasserstrom unter dem Einflusse der stärker gereizten secretorischen Fasern."[23]

There are two possible fallacies in Heidenhain's argument. One fallacy probably lies in his tacit assumption that the gland secretes as a whole; that the secretion following a strong stimulus is derived from the same alveoli as the secretion following a weak stimulus. The other fallacy is the assumption that all of the organic constituents of saliva secreted from a fresh gland upon a strong stimulus are in solution. The true reason why the dilator-secretory nerve may cause an increase in the organic matter present in a secretion, coincident with an increased rate of flow, in passing from a weak to a strong stimulus, may be the following:

If a very weak stimulus be used, only a portion of the alveoli are aroused to activity. The supply of stored up products (hylogens) in these, becomes soon exhausted and the secretion derived from them is poor in organic constituents. On passing to a strong stimulus, the previously resting alveoli are thrown into activity and the secretion derived from them is rich in organic constituents. It is the secretion from these fresh alveoli, which increases the percentage of organic constituents in the whole secretion. On passing from a long continued weak to a strong stimulus in a fresh gland, one is really passing from an exhausted to a fresh portion of the gland.

Moreover, in Heidenhain's observation there is a second source of error which he has overlooked. Heidenhain treats all of the organic constituents of the rapidly secreted saliva as if they were in solution and considers that the liquid derived from the blood is in contact with the materials to be dissolved, only during the time of its passage through the cell. There can be little question, however, that saliva, and particularly the rapidly secreted saliva of a fresh gland, cannot be considered a true solution, for it contains many bodies in suspension. Heidenhain himself has been one of those to describe the microscopical appearance of the lumps of mucous matter, salivary corpuscles and occasional leucocytes found in this secretion. The presence of these bodies in saliva indicates that the rapidly secreted saliva carries out of the cell not only substances in solution, but viscous masses of mucous matter not in solution. Its swift cur-

rent is able to transport these masses, while a more slowly flowing secretion is not. Furthermore, in all probability the saliva keeps on dissolving them as it carries them along and hence becomes actually more concentrated, because it is in contact with them really for a longer time than the more slowly secreted saliva and not for a shorter time as Heidenhain thought. Heidenhain made no endeavor to distinguish between the matters in suspension and those in solution.

That any gland functions as a whole, as Heidenhain tacitly assumes in his explanation, can not be maintained.

The whole surface of the stomach, for instance, may be considered as one large gland. It has long been known that secretion can ensue in one spot, and not in another. Heidenhain himself, has called special attention to the marked differences in the condition of the various alveoli in the salivary glands. Even in the resting gland, here and there alveoli will be found possessing the structural features of secretory activity.[22] In the stomach he remarks that some glands show changes on stimulation before others,[23] and I have, myself, repeatedly observed glands in the Newt's stomach close together in very different stages of activity. Kühne and Lea[33] have observed this in the living rabbit's pancreas, a portion only of the gland being normally active. After pilocarpine all the alveoli passed into a condition of activity. In the kidney the independence of the various tubules in secretion has been remarked for the bird's kidney by von Wittich, and for the mammalian kidney by Ribbert,[64] and by Dr. Herter in conjunction with the author. Finally, in the case of the salivary glands, Langley says that even on prolonged activity of the chorda many alveoli show no change. "This is due, in some cases, to fibres escaping stimulation, fibres which leave the lingual later than usual." This histological evidence appears to me to be conclusive with reference to the idea that the gland does not function as a whole, but that the individual alveoli in the secreting gland may be here active, there passive.

The physiological evidence that the foregoing is the true explanation of Heidenhain's observation is hardly less conclusive. We can easily obtain evidence that the secretion obtained

during a weak stimulus is derived from a portion of the gland only in the following manner: Let us stimulate the chorda nerve carefully with a very weak current, until a large amount of secretion has been obtained. If this secretion has been derived from the whole gland a stronger stimulus should yield a secretion much less concentrated than a stimulus of equal strength before the weak stimulus. The glands should show, in other words, a considerable exhaustion of the gland products. If, on the contrary, the whole of this secretion has been derived from a portion only of the gland the rest of the alveoli must remain practically unaltered, and a stronger stimulus arousing these should yield a juice, little, if any, poorer in organic matters than was yielded by a stronger stimulus before the weak.

Werther[75] has unintentionally tried this experiment and found the latter possibility to be what actually occurs. A very weak stimulus, with the secondary coil at 300–240 mm., was employed for over three hours, and more than 20 cc. of saliva were secreted. The percentage of organic solids secreted in the slowly flowing saliva steadily fell, but the percentage of such bodies in the saliva secreted on a succeeding stronger stimulus was little if any less, after this long secretion, than it was with an equally strong stimulus before. If, however, a somewhat stronger stimulus was employed, the secretion from a still stronger stimulus was much poorer in organic solids, than the similar stimulus before the weak.

The fact that rapidly secreted saliva is not a pure solution, and the considerations just presented concerning the independence of the alveoli of the gland render this observatoin of Heidenhain of doubtful value as evidence of the existence of secretory nerves.

Moreover, there is good reason for doubting the truth of Heidenhain's statement, in the quotation on page 333, that the liquid derived from the blood is incapable of dissolving the constituents of the cells in the absence of nerve influence. As has already been pointed out, in treating of sympathetic saliva, (page 322), if the thin chorda saliva be simply left in the gland for twenty minutes, or more, it is converted into a dense, vis-

cous fluid having all the characteristics of sympathetic saliva. This conversion takes place with equal readiness whether the gland nerves be intact or divided.

Heidenhain's own explanation, also, will be found on analysis, I believe, to involve such assumptions as to arouse serious doubt of its truth. To explain this phenomenon on the basis of secretory cell activity, he assumed separate "trophic" nerve fibers acting on the cells. He thus necessitated the improbable conclusion, that at least many of the cells of the submaxillary gland received at least four different nerve ends, *i. e.*, trophic and secretory of the sympathetic, and trophic and secretory of the chorda; and at least two entirely different nerve impulses, *i. e.*, trophic and secretory. That such a consequence should not have aroused suspicion in his own mind of the truth of his explanation is difficult to understand.

b. Post-mortem Chorda Salivary Secretion.

Another strong argument that the chorda does not produce its secretion by its dilator action on the blood vessels, but by direct action on the gland cell, has been derived from the so-called post-mortem chorda secretion. Ludwig and Heidenhain found that if the gland's artery be completely closed, or if the head be rapidly cut off, and the chorda at once stimulated, a fairly copious secretion ensued. This secretion was most abundant in the first minute after section, and thereafter rapidly diminished, but a little could still be obtained four, and in some cases five, minutes after decapitation, or compression of the artery. Thereafter the nerve was ineffective. Heidenhain believed this secretion to be due to the action of the nerve on the gland cell, and its rapid failure to lack of oxygen and water. Both Ludwig and Heidenhain believed that by the conditions of the experiment they entirely eliminated the factor of the nerve's vaso-motor action, and hence thought it demonstrative evidence that the secretory and dilator functions of the nerve were independent.

I think it may be questioned, however, whether the conditions of the experiment do entirely obviate the vaso-motor action of the nerve, and whether it is not still possible that this dila-

tion may cause the secretion. It is conceivable that this post-mortem secretion might be due to the flow of blood from the veins and arterioles into the capillaries, owing to the active dilation of the latter during chorda stimulation. This explanation, it is true, necessitates the assumptions that the chorda tympani causes, on stimulation, an active dilation of the capillaries, or veins, as well as of the arterioles, and that that dilation in some manner makes it easier for the liquid to pass out into the secretion. Both of these assumptions are difficult of proof, and in the limited time at my disposal I have not been able to get demonstrative evidence, either of their truth or error. There is some reason to believe, however, that they may possibly be true.

That liquid passes out of the capillaries into the secretion of the submaxillary gland because of an attractive pull exerted upon it by some constituents of the gland cells, has been suggested both by Ludwig and Heidenhain. To the evidence presented in favor of such a view by Heidenhain, I have nothing to add, and in the normal condition of the capillary and gland wall, I presume that the hypothesis is true. Ludwig supposed that during chorda stimulation the attractive pull of the cell was increased, owing to the formation of substances in the cell possessed of a higher endosmotic equivalent. Heidenhain believed that the attraction of the cell for the liquid in the blood was constant, but that on stimulating the chorda, the turgor of the cell diminished owing to the passage of liquid into the gland lumen, and water was thus enabled to enter the cell from the blood. Both of these explanations, as will be noticed, assume that in some manner the effectiveness of the attractive pull of the cell is increased during nerve stimulation and water enters the cells independent of the state of the vascular system. The question which confronts us and which it was supposed this post-mortem secretion settled is this: Does stimulation of the nerve cause secretion by increasing in some manner the attractive pull exerted by the gland cells on the liquid of the blood, or does it indirectly render effective by vaso-dilation an attraction which is constantly exerted by the cell on this liquid? This is a very difficult point to determine. The endeavor

has been made to answer this question indirectly by showing that vaso-dilation may ensue without secretion, and secretion without vaso-dilation. But all the evidence which has hitherto been offered, that vaso-dilation may ensue without secretion, and that it alone is incapable of causing secretion, is invalidated by the fact that the conditions of such experiments produce an abnormal gland, or capillary wall, both factors which research on lymph formation have shown to be of importance. Quinine, hydrochloric acid, sodium carbonate, or atropine, drugs which enable vaso-dilation to ensue without secretion, probably alter the permeability of the capillary, or gland cell. So that inferences can be drawn from such experiments as to processes occurring in the normal gland only with the greatest caution. The evidence with the exception of the post-mortem secretion, that the chorda may cause a secretion without vaso-dilation is also unsatisfactory, as pointed out on p. 355. Attention may now be directed, hence, to this post-mortem chorda secretion.

It is probable from the considerations presented on page 338, that the liquid causing this secretion is derived from the blood. Can the chorda tympani act on the blood vessels in the absence of circulation, in such a manner as to facilitate the passage of that liquid from the capillaries to the gland cells? The only possible way in which it might so act, I believe, is by causing an active dilation of the capillaries or veins, as well as of the arterioles. Is there any evidence that the chorda has such an action?

Tiegerstedt[68a] states that the capillaries are contractile but that they have not hitherto been shown to be under nerve control. Roy and Brown have brought forward strong evidence that the capillaries are normally in a state of tonic contraction and that they may actively expand independent of the blood pressure. They observed in the capillaries of the web of the frog's foot that, although blood pressure might be diminished almost to atmospheric pressure, the application for an instant of chloroform to the web caused an enormous expansion of the capillaries. Interesting, also, in this connection, are the observations of von Frey. v. Frey[17] examined microscopically the capillaries of the

frog's tongue. He found that on stimulation of the dilator, hypoglossal nerve, a dilation of the capillaries ensued even after the blood supply had been cut off. If the artery be clamped, he observed that the blood streamed out of the capillaries both into the arteries and veins. If, now, the hypoglossal be stimulated the capillaries dilate and blood streams into them from the arterioles and veins. This movement persisted for from one to two minutes after clamping the artery. Furthermore, in experimenting on the blood flow from the veins of the submaxillary gland of the dog during stimulation of the chorda, v. Frey often observed that stimulation of the chorda was followed by a temporary decrease in the rate of flow of blood from the vein, before the ordinary increase. He suggests that this would seem to indicate a widening of the capillary area leading to a back flow of blood from the veins were it not more probable that the increased flow from the dilated arterioles would be more than sufficient to offset this.

These facts justify the conclusion, I believe, that on stimulating the chorda tympani in the severed head, the capillaries of the gland probably dilate, and that blood enters them from the veins.

How such a vaso-dilation might lead to a secretion is not clear, but two possibilities suggest themselves: (1) that the capillaries are thus brought into closer relation with the alveoli, and the constant attraction exerted by the gland contents for the water of the blood is thus rendered effective; or (2) that vaso-dilation may in some way increase the permeability of the capillary wall. The post-mortem chorda secretion can not, I believe, be accepted unconditionally as illustrative of a secretion independent of vaso-dilation, until these possibilities have been shown to be non-existent, or non-essential.

If it shall be found that vaso-dilation of itself is a cause of secretion in the normal gland, and that the gland cell is not the secretory agent, the facts of secretion in the submaxillary gland will probably necessitate the following conclusions, which are not without interest for those studying the physiology of the circulation: (1) That stimulation of the chorda causes an ac-

tive dilation of the capillaries, as well as a dilation of the arterioles. (2) That the sympathetic is able to overcome the chorda's action on the arterioles, but not its action on the capillaries. This is shown by the following fact: If, during strong stimulation of the sympathetic, the chorda be irritated by a current which by itself is barely able to arouse a secretion, a secretion ensues which is certainly as large, if not somewhat larger, than the chorda alone would cause. Such a weak stimulus of the chorda is, however, unable to neutralize the sympathetic's constrictor action on the arterioles, as shown by the observations of v. Frey. It will be necessary to assume, hence, that the arterioles have remained contracted, while the capillaries have dilated and blood has entered them from the veins producing a secretion analogous to the post-mortem chorda secretion.

I endeavored, in a variety of ways, to obviate with certainty all possibility of the chorda's dilator action. By the injection of supra-renal extract into the circulation I hoped to cause such an intense peripheral constriction as to neutralize the dilator action of the nerve. I am indebted to Dr. R. H. Cunningham for this suggestion. After division of the chorda I injected into the jugular vein the whole of a normal salt extract of two powdered supra-renal capsules of another dog. I found, however, that the injection was followed by a slow constant secretion of what appeared to be sympathetic saliva, and that this secretion was increased at all times by a very weak stimulation of the chorda. Indeed, the chorda caused a larger secretion after the injection than before, probably due to the vaso-constriction in other areas of the vascular system. This result was so discouraging that I did not attempt to repeat it.

Heidenhain remarks that large doses of physostigmin cause such an intense constriction of the arterioles of the gland after division of the chorda that stimulation of the latter nerve is unable to cause either a vaso-dilation, or secretion. Unfortunately, Heidenhain does not give a full account of the experiment. Were it true that the drug produces this effect within three or four minutes of its injection, it would be, I believe, conclusive evidence that secretion can not ensue in the absence of vaso-

dilation, and that the nerve does not cause secretion by action on the gland cells; for it is known that the drug does not directly paralyze the hypothetical secretory fibers, or the gland cell. To obtain the details of the drug's action, I injected into the jugular vein of a medium-sized dog 0.1 gr. of physostigmin sulphate. But although the chorda was divided, a spontaneous secretion began which stimulation of the chorda considerably increased. This discrepancy from Heidenhain's results is probably due, I believe, to the impure calabar extract he used.

I endeavored to ascertain whether the presence of blood in the capillaries was an essential condition of the post-mortem secretion by forcing the blood out with air. After ligaturing the carotid artery and placing in it a canula directed headwards I rapidly cut off the head and allowed air to pass into the carotid under a pressure of 100 mm. of Hg. The first experiment gave a positive result. On stimulating the chorda a brief, scanty secretion was obtained which quickly ceased. Examination of the gland showed it to be practically bloodless. In two other similar experiments the post-mortem secretion was greatly reduced in amount and ceased after 1 to 3 minutes, instead of lasting for from 3 to 5 minutes, as normally. The glands in these experiments still contained blood in the veins. The experiments indicate, I believe, that the presence of blood in the capillaries is an essential condition of this secretion. I regret not having been able to bring my experiments to a more satisfactory conclusion, but it is to be hoped that the important bearing of this post-mortem saliva upon the theory of secretion may lead to its being made the subject of careful investigation.

From the following experiments the following conclusions may be drawn relative to this post-mortem secretion:

1. After clamping the gland artery, or cutting off the head, a secretion may be obtained from the submaxillary gland on stimulating the chorda. This secretion is most abundant in the first minutes, and thereafter rapidly diminishes. After four or five minutes no more secretion can be obtained. The total amount of saliva secreted varies from 0.3 to 1.5 cc. (Experiments XVIII, XXII and LXIV.)

2. If the gland be left without stimulation for a minute after decapitation the total amount of saliva obtainable is considerably reduced.

3. If the gland be not stimulated until 3 or 4 minutes have passed a small secretion may be obtained 6 minutes after decapitation. (Experiment XVIII.)

4. If air be blown into the carotid artery, after cutting off the head, the secretion of saliva is reduced in amount and secretion ceases, either abruptly or after 2 to 3 minutes. (Experiments LXIII, LXVI and LXVII.)

5. If defibrinated blood be run under small pressure into the vein of the gland a small secretion may be obtained 20 to 30 minutes after clamping the gland artery.

6. If the blood supply be cut off for 30 minutes, on readmitting blood the arterioles dilate, arterial colored blood issues from the vein at a rapid rate and a spontaneous secretion begins. The rate of this secretion is not changed by stimulation of the chorda in the first minute. (Experiment Va.)

Experiment Va.

Large dog. 3 cc. 1% morphine sulph. subcut. Tracheotomy. Ether. Canulæ in both submaxillary ducts. Both chordo-linguals and both sympathetics cut. The left vagus subsequently divided also. The right gland is stimulated from time to time. See p. 305. The left is freed from its tunic and is attached only by the hilum. The vein on the upper surface is open and flows continuously. The only blood vessel coming to the gland is the hilum artery. The other artery was tied and cut.

Readings computed in cc.

Time.						Nerve.	Amount of Secretion in cc.
h	m	s	h	m	s		
3	25					Clamped artery going to gland.	
3	25		– 3	30		c	Gradually less.
3	30					c	None.
3	32					c	"
3	35					s	.07

3	37					s	.00
3	40					s	.00
						Inject 5 cc. .5% NaCl into duct.	
3	41					**s**	**.05**
3	42					Unclamped artery.	
3	43	30				c	Active secretion.
3	44					Gland secretions spontaneously .17 cc. per minute.	
						Cut left vagus.	
4	07	30				Clamped artery again.	
4	07	30	– 4	08		Chorda (intermittent).	.50
4	08		– 4	09		c	.18
4	09		– 4	11	30	c	.07
4	12					c	.00
4	13		– 4	14		**s**	**.08**
4	15		– 4	17		c-coil 12	.00
4	17	30	– 4	18	15	**s**	**.05** (very viscid)
4	20					s	.00
4	23					Inject NaCl. 5% into duct.	
4	24					s 30 sec.	**.04**
4	25					c	.00
4	26		– 4	27		s	.00
4	28					Inject ½ cc. fluid into duct. Most of it runs out before stimulation.	
4	29					**s**	**.025**
4	29	30				Unclamp artery (red blood rushes out of vein).	
4	30		– 4	31		*Gland secretes spontaneously.*	.1 cc.
4	31		– 4	32		" " "	.12 cc.
4	33	6				c	.30 cc. per minute.
4	35		4	32		Spontaneously secreting.	.08 cc. per minute.
4	37		– 4	38		c 1 mm.	.7 cc.
4	38		– 4	45		Spontaneously.	.5 cc.
4	45					c	.9 cc. per minute.
4	45	30				Clamped artery again.	
4	46	30	– 4	47	30	c (coil 12)	.5
						Gland still slowly secreting spontaneously.	
4	48	30	– 4	49	30	c	.1
4	50		– 4	51		c	.03
4	51	30	– 4	52	30	c	.005 in first thirty seconds, then no more.
4	53		– 4	54		**s**	**.03**
4	54		– 4	55		c	.00
4	55	30	– 4	56	30	c coil 10	.00
4	57		– 4	58		s coil 10	.015
5	02					Unclamped artery.	
5	02	30				c	Readily secretes.
						Blood rushes continuously out of vein a bright red on unclamping the artery.	
5	03		– 5	09		*Gland secretes spontaneously .5 cc.*	
5	09		– 5	10		s	.05

5	13	30					Clamped artery.	
5	13	40 –	5	14	40		c	.5
5	14	40 –	5	17	30	No stimulation.		
5	17	30 –	5	18	30		c	**.03**
5	19	–	5	20			c	.02
5	20						c	.00
5	22	–	5	23			s	.01
5	24						c	.00
5	25	–	5	26			s	.01
5	35						c	.00
5	35	30					s	.00
5	36					Unclamped artery. Red blood rushes from the vein.		
5	40					Chorda. Rapid secretion.		
						Gland secretes spontaneously.		
5	45	–	5	46		Right Sympathetic.		.1 cc.
5	47	–	5	48		Left Sympathetic.		.04 cc.
5	49	30				Cut off head as rapidly as possible. Was unable to saw through the vertebral column. All the muscles and skin severed.		
						Right gland.		
5	50	30 –	5	55		Intermittent stimulation of right chorda.		.530
5	55					Chorda (coil 5) muscular contractions. No secretion.		
5	57					Right sympathetic.		**.22 cc.**
6	10					Right sympathetic.		**.04 cc.**
5	56					Left gland; no secretion either from chorda or sympathetic.		

Experiment LIV.

Right submaxillary. Chorda and sympathetic cut. Dog under morphine and ether. Tracheotomy. The dog's respirations become very slow, and finally cease without any struggles, and without ether. There was considerable fluid in the trachea.

4.46. Stimulate the chorda while dying, chorda effective until 4.50. The secretion becomes less and less and finally ceases.

I then stimulated the sympathetic and obtained a very copious secretion of .2 cc. No more secretion from either nerve.

Experiment LXIV.

Before cutting. 10 seconds stim. Coil 24. Secretes .79 cc.

Begin to cut at 4.50. 1 minute to sever head completely. No secretion during operation.

h	m	s	h	m		AMOUNT.
4	57		– 4	58	Stimulates 3 times, 10 seconds at a time.	.515 cc
4	59				" 10 seconds	.150 cc.
4	59	30			" 10 "	.021 cc.
					No more secretion.	
					Total time of stimulation 50 seconds. Total amount.	.686 cc.

From beginning to cut to end of chorda effect, 3 m. 30 s.

Experiment XXI.

Before cutting. Coil 20. 10 s. stimulation secretes .55 cc. Begin to cut at 4.05. 1 minute to sever head completely. No secretion during operation.

h	m	s	h	m			AMOUNT.
4	06		– 4	07	Stimulate 3 times, 10 seconds at a time.	1.	.235
						2.	.040
					Dog swallows.	3.	.090
4	07		– 4	08	" 3 times, 10 seconds at a time.	1.	.070
						2.	.040
					Swallows.	3.	.060
4	08	15			Coil to 10, muscular contractions, 10 sec.		.100
4	09				30 seconds stim. off and on (muscle).		.030
4	09	15			No more secretion.		
4	10				Coil 4. Heavy contractions (escape of current).		000
					Total time of stimulation, 85 seconds. Total amount,		.665 cc.

Time from beginning to cut until end of chorda effect, 4 m. 15 s.

Experiment XVIII.

Before cutting. Coil 11. Stimulate 10 seconds.
Right gland secretes .64 cc. Left gland, .61 cc.
5.24.30 begin to cut head. Head severed in 30 s.

h	m	s	h	m	RIGHT GLAND.		AMOUNT.
5	25		– 5	26	Stimulate 3 times, 10 seconds at a time.	1.	.125
						2.	.100
						3.	.080
5	26		– 5	27	" 4 " " "	1.	.070
						2.	.050
						3.	.020
						4.	.010
5	27		– 5	28	" 40 seconds.		.040 cc
5	28	30			" 10 "		.000

		Left Gland.	Amount.
5	30	Stimulate left chorda 10 seconds.	.070
		next 10 "	.010
5	30 30	" " chorda (strong muscular contractions).	.070
5	31	Left chorda. No more effect except on muscular contraction.	

Summary.

Right gland.

Total time of stimulation, 120 seconds. Total secretion, .495 cc. From beginning of cut to end of chorda effect, 4 minutes.

Left gland.

Total time of stimulation, 20 seconds. Total amount, .080 cc. Time from beginning to cut to end of chorda effect (2) 5 minutes, 30 seconds.

Experiment LXIV.

Before cutting. Coil 18. 30 sec. stimulation. Secretes 2.1 cc. Cut head at 4.30, 1½ minutes to sever completely.

h	m	s		h	m		
4	31	40	–	4	36	Intermittent stimulation. Secretes No more secretion after 4.35.	.250 cc.
4	38					Stimulate sympathetic for two minutes, secretes	.065 cc.

Time from beginning of cut to end of chorda effect 5 minutes.

Experiment XXII.

Before cutting. Coil 18. 10 sec. stim. Secretes .2 cc. Cut at 6.07. 30 seconds to sever head completely.

h	m	s		h	m	s		
4	07	30	–	6	9.		Stimulation, 1st 10 seconds	.225 cc.
							40 seconds stim.	.060 cc.
6	09	20	–	6	19	10	Stimulate coil 18. 30 sec. stim.	.150 cc.
6	10	30					Chorda no mre effect	
6	12						Coil to 14. Muscular contractions	.050 cc.
							Total secretion	.375 cc.

Time from cuttiug till chorda ineffective, 3 m. 30 s.

Experiment LXIII.

Small dog, Irish terrier, under ether. Canula in left Wharton's duct. Tracheotomy. Chorda-lingual nerve cut. Pro-

tected electrodes on chorda. Vago-sympathetic not cut. Canula connected with air reservoir in the head end of the left carotid artery.

Before cutting, stimulation of the chorda, with secondary coil at 200, causes a secretion of 0.15 cc. in 10 seconds.

Head rapidly severed at 4.17 P. M. As soon as it was severed I opened the cock, letting air into the carotid. I then stimulated the chorda tympani at 4.18. Stimulation of the chorda causes a secretion of .02 cc. Secretion then stops and no more can be obtained by any strength of stimulus.

Experiment LXVI.

Conditions of the experiment as in Experiment LXIII. Before cutting off the head stimulation of the chorda for 10 seconds with secondary coil at 180 causes a secretion of .17 cc.

Head rapidly severed from body at 3.03. Chorda stimulated at 3.03.45 for 20 seconds. Gland secretes .20 cc. Air then forced into the carotid artery.

3.04.30–3.05.30 stimulation of the chorda with secondary coil at 130 causes .07 cc. Thereafter no secretion with a stimulation of any strength.

Experiment LXVII.

Conditions of experiment the same as in Experiment LXIII. Before decapitation stimulation of the chorda for 10 seconds with secondary coil at 230 yields a secretion of 0.2 cc.

Dog decapitated at 10.49. Air forced into carotid as soon as cutting began. Head severed in 30 seconds.

h.	m.	s.					
10	49	45	Chorda	10 seconds.		Coil 230	0.1 cc.
10	50	30	"	"	"	" 200	0.05
10	52		"	20	"	" 180	0.05

Thereafter no more secretion.

Post-mortem examination shows the gland veins to be filled with blood. The air does not seem to have penetrated the gland.

c. The Nature of the Action of Atropine and Pilocarpine.

Atropine permits vaso-dilation, on stimulation of the chorda, but prevents secretion. The drug has been supposed to act, not on the gland cell, but on the ends of the secretory nerve fibers. The reasoning for this is as follows: In the dog's submaxillary, atropine paralyzes the chorda secretion, but not the sympathetic. If the sympathetic innervate the gland cell and cause its secretion by action on the latter, the gland cells connected with this nerve have evidently not been paralyzed. As there is no reason to suppose these cells different from those connected with the chorda, it is probable that the cells connected with the chorda have not been paralyzed. But if the gland cells have not been paralyzed, and the dilator action of the nerve remains unaffected, we must assume that there is some third element connected with the nerve which has been paralyzed. This must be the element causing secretion, *i. e.*, the secretory nerve fiber. The latter must be paralyzed at the nerve termination, since, as far as known, atropine does not act on the nerve fibre. This argument is true only for the dog and not for the cat[35] since, in the cat, atropine paralyzes the sympathetic as well as the chorda. The argument, as will be seen, depends on the assumption that the sympathetic causes secretion by action on the gland cells. This, as pointed out, is probably incorrect. The sympathetic produces its secretion by action on contractile tissue. There is, hence, no longer any reason to suppose that the gland cells have not been paralyzed by the drug. How it acts upon the cell is unknown, but the effect of that action is to prevent or diminish the passage of fluid through the cells. The variation in the susceptibility to its action of different glands in the same animal (compare the pancreas, salivary glands and kidneys of dog), or of the same gland in different animals (compare the pancreas of the dog and rabbit) points, I believe, toward an action on the gland cell itself, the variations in its action being due to variation in the chemical composition of the cells.

That atropine does act on the gland cell is, perhaps, indicated also by the action of its great antagonist pilocarpine. Pilocarpine, namely, produces a secretion of sweat two to three weeks after cutting the sciatic of the cat, when the nerve is totally inactive.[72 52 46] Luchsinger,[47] in commenting on this, says that this secretion must be due either (1) to action on the secretory cells themselves, or (2) to the non-degeneration of the nerve ends. The second possibility is impossible since these nerve ends are not provided with nuclei. A similar secretion may be obtained in the dog's salivary glands, fourteen days after cutting both chorda and sympathetic. The evidence is here not so conclusive since the submaxillary ganglion does not degenerate. In the sweat secretion, however, I believe the evidence is fairly strong that pilocarpine does act directly on the gland cell. It thus strengthens the evidence that atropine also acts on the cell.

There is also reason for believing that atropine acts in some manner on the capillary wall, thus reducing, or preventing the transudation of lymph. It might, in this way effect secretion from glands. This possibility has not received the attention it deserves.*

The evidence that atropine checks lymph transudation is as follows:

If atropine permitted the transudation of lymph normally ensuing on vaso-dilation, it would be expected that, after its injection, stimulation of the chorda would render the submaxillary gland œdematous, since fluid no longer passes into the secretion. Quite the contrary is the fact. I have repeatedly stimulated the gland all day, after the injection of atropine, without producing a trace of œdema. Heidenhain[25] himself says: "After atropine on stimulation of the chorda tympani no in-

* Heidenhain's reasons for rejecting the possibility that atropine checks lymph transudation and thus secretion will be found in Hermann's Handbuch. A striking instance of failure to consider this possibility is the following quotation from Langley:

"Atropine prevents the stimulation of the hilum from producing a secretion. Nicotine does not do this, therefore, atropine acts upon structures more peripheral than those acted upon by the nicotine. Since nicotine acts on nerve cells, and atropine does not act on gland cells, atropine must produce its paralyzing result by action on the secretory nerve endings."

crease in lymph flow occurs, even when during stimulation of the chorda the medulla is stimulated and the blood pressure greatly increased." Brunton in commenting on this says: "It appears to me that this circumstance can hardly be explained otherwise than by supposing that atropin not only paralyses the secretary fibres of the chorda, but acts upon the blood vessels in such a manner as to greatly diminish or prevent the exudation which would usually take place from them into the lymph spaces."

Heidenhain[23] supposed that lymph normally left the blood vessels on account of the secretory pull exerted by the gland cell. Atropine prevented lymph transudation by paralysis of the secretory chorda nerve ends. He was led to this conclusion chiefly by the following facts: (1) No more lymph normally leaves the blood vessels than passes into the secretion, and (2) if one inject 4.9% solution of sodium carbonate, 0.5% hydrochloric acid or quinine sulphate into Wharton's duct the chorda's secretory power is annihilated, but on stimulation the gland becomes highly œdematous. If, however, atropine be injected into the blood before the chorda is stimulated and after the injection of quinine into the duct no œdema ensues, however long the nerve be stimulated. I have fully confirmed these observations. The most probable interpretation of these facts, it seems to me, is that quinine prevents the passage of fluid through the glands by action on the gland cells, but does not prevent lymph transudation. That atropine, however, acts directly on the capillary wall, as well as upon the gland cell, in such fashion as to prevent lymph transudation and secretion.

A further indication that atropine checks lymph transudation is the diminution in thoracic lymph flow after its injection. Tschirwinsky[69] found that in morphinized animals thoracic lymph flow fell from 3.75 cc. to 1.5 cc. and from 10 cc. to 4.2 cc. in a given time. Atropine neutralized, also, the increased flow due to curare. In the latter case it fell from 9 and 10 cc. to 2.5 and 5.3 cc. in a given time. As there is reason to believe (Adami) that curare increases lymph transudation by direct action on the capillary wall, the inhibiting action of atropine may

be referred to an opposite action on the same structure. Not knowing of Tschirwinsky's work, I had already performed similar experiments on the lymph flow, comparing it with pancreatic flow on vagus stimulation and after pilocarpine. I found (Experiment V that atropine temporarily neutralizes the large increase in lymph flow which occurs concomitant with increased panceas secretion during rythmic stimulation of the vago-sympathetic after division of the cervical cord, and also neutralizes the increased lymph flow due to pilocarpine.

Experiment Vb.

Medium-sized dog. Ether. Temporary pancreatic fistula. Tracheotomy. Cervical cord cut. Artificial respiration. Thoracic duct prepared. Lymphatics of head and neck ligatured. Readings every minute in cubic centimeters :

Thoracic Duct.	Pancreas.	Thoracic.	Pancreas.	Thoracic.	Pancreas.
Vagi uncut.		.050	.009	.197	.009
.220	.02	.110	.013	.180	.004
.220	.02	.115	.012	.180	.008
.200	.02	½ hour interval.		.150	—
.200	.015	.120	.013	.190	.006
.180	.015	.119	.012	Rt. Vagus. Ryth. Coil 9.	
.180	.010	.090	.009	.200	.000
.190	.015	.130	.011	.180	.000
.160	.013	.120	.010	.100	.002
.180	.017	.110	.010	**.300**	**.068**
.155	.017	.100	.008	—	.025
.155	.018	.100	.009	—	.015
.170	.015	.102	.004	—	.015
Cut vagi in neck.		.100	—	Off.	
.280	.015	Clot.		.160	.020
.220	.010	1 shock per second.		.140	.010
.160	.005	Rt. Vagus. Ryth. Coil 10.		.150	.005
.100	.003	.150	.009	—	.010
.120	.007	.220	.006	Rt. Vagus. Ryth. Coil 9.	
.100	.009	.250	.010	.360	.006
.100	.010	.170	.005	.200	.009
.060	.015	.230	.010	.240	.005
.050	.020	Current off.		Coil to 4.	
.090	.010	.200	.005	.200	.005
.120	.015	.190	.007	—	.008
.120	.003	.220	.007	Off.	
.120	.007	.220	.005	Clot.	0.15
.065	.010	.310	.002	"	.011
.125	.010	.210	.001	Left Vagus. Ryth. Coil 9.	
.100	.011	.180	—	.550	.030

Thoracic.	Pancreas.
.290	.005
—	.005
—	.005
—	.010
Coil to 6	.015
—	**.060**
—	**.090**
.240	**.100**
—	**.090**
Off. Then on by accident.	
.230	.060
Off.	
.140	.035
.170	.030
.160	.030
.120	.015
.170	.015
.130	—
.170	.010
Left Vagus. Ryth.	Coil 6.
.140	.007
.130	.002
.060	.009
.140	.090
.200	.120
—	**.130**
.290	**.140**
.235	**.110**
.280	**.130**
.235	**.130**
.250	**.080**
Off.	
.340	**.154**
.210	.116
.190	.052
.160	.043
.190	.020
.190	.020
.170	.025
—	.011
—	.014
.155	.015
—	.010
.150	.010
.150	—
Left Vagus. Ryth.	Coil 6.
.210	.010
.190	.000

Thoracic.	Pancreas.
.120	.005
—	.010
.175	.025
.225	.045
.250	**.055**
.220	**.110**
—	**.120**
.320	**.140**
.300	**.130**
Off.	
—	.115
.170	.065
.200	.030
.150	—
.200	.030
—	.020
—	.015
.140	.015
.140	.010
.145	.016
.135	.009
.130	.005
.160	.017
Left Vagus.	Rythmical.
.160	.002
.250	.008
.210	.000
.240	.001
—	**.015**
.300	**.075**
.300	**.035**
.270	**.045**
—	**.100**
.350	**.100**
.300	**.110**
—	**.140**
Left Vagus. Ryth.	Coil 6.
.220	**.110**
.280	**.070**
Off.	
.200	.070
.200	.050
.230	.020
.180	.025
.180	.030
.155	.005
.145	.005
Left Vagus. Ryth	Coil 6.

Thoracic.	Pancreas.
.270	.015
—	.005
.230	.005
.240	.000
— suddenly	.120
.250	.080
Inject .5 cc. atropin into supra-scap. vein	
Stimulation continued.	
.250	.050
.200	.070
—	.030
.180	.020
.140	.015
.145	.015
.155	.015
.110	.010
.120	.015
— Off.	.007
.120	.008
.130	.010
.5 cc. atropin.	
.080	.010
.040	.010
.090	.005
.100	.007
.100	.008
.120	.012
.100	.007
.100	.005
.110	.005
—	.006
Stim. Left Vagus.	Coil 6.
.070	.010
.100	.008
.120	—
.160	.002
.160	.000
.230	.000
.200	.000
.260	.000
.170	.000
.200	.000
Off.	
.250	.000
.170	.000
.190	.000

This experiment is of interest, not only as a clear confirmation of Pawlow and Mett, but because of the invariable increase in thoracic lymph flow occurring on stimulation of the vagus. I have repeatedly sought to obtain other experiments like it, but never with such success. The operation is long and apt to miscarry at some point.

Experiment XI.

Dog, etherized. Canula in thoracic duct. Readings in cc. every minute.

Thoracic duct.

.150, .220, .200, .180, .300, .230, .250.

1 cc. 1% pilocarpine into left femoral vein. Dog perfectly quiet.

.250, .300, .500, .600, .400, .460, .400.

1 cc. pilocarpine.

.490, .410.

1 cc. 1% atropine 1%.

.240, **.090, .060, .070, .170, .110,** .120, .090, .090.

Moved head.

.220.

1 cc. atropin.

.130, .100, .070, .060, .040, .120.

2 cc. pilocarpine.

.100, .080, .120, .130.

1 hour interval.

.160.

It is not without interest in this connection that pilocarpine, contrary to atropine, increases lymph flow. This was first observed by Tschirwinsky.[69] My own experiments have yielded a positive result generally, but not invariably. In all cases the dogs had divided cervical cords, and generally divided vagi. They were all under artifical respiration. The lymph was measured in cc. for equal intervals of time.

Experiment.	Before pilocarpine injection.	After the injection of 1–2 cgs. of pilocarpine.	Remarks.
11	1.53	3.00	7 minutes. Dog motionless.
29	2.44	6.09	Some movements of abdomen.
14	0.50	1.72	Motionless. 9 minutes.
4	1.55	10.40	Movements.
62	1.41	1.69	No movements. Pancreas did not secrete either.

In experiments 11 and 14 there were no visible movements. The flow of the seven minutes after injection in No. 11 was

double that of seven minutes before, and in experiment 14 was three times as great. In experiment 62, however, there was scarcely any difference.

The evidence presented in the foregoing pages, if not conclusive, certainly indicates that atropine restricts and pilocarpine increases lymph transudation. They may in this manner affect secretions. In any case, if the sympathetic causes its secretion by action on contractile tissue in the gland, there is no longer any reason against assuming that atropin acts directly on the gland cell, in such manner as to check the passage of fluid through it, and thus to prevent secretion.

d. The Action of Quinine and Nicotine.

We have considered the three main objections which have been raised against the chorda salivary secretion being an osmosis. There are, also, certain other phenomena which have been thought indicative of the independence of the secretory and dilator action of this nerve, and, hence, are worthy of a short criticism.

The first is the action of quinine, which when injected into the gland duct causes a temporary vaso-dilation, but no secretion. If, however, the chorda be stimulated, still greater dilation ensues and secretion takes place. This secretion is less than normal. Heidenhain[21] interprets this to mean that vaso-dilation cannot of itself produce a secretion, but that the secretory fibres must be aroused. (See literature reference No. 21, p. 85. Also reference No. 23, p. 45.)

The facts may, however, be otherwise understood. Quinine prevents the passage of liquid through the gland cell. This is shown by the fact that ultimately it prevents chorda secretion, even though the gland become œdematous. If the permeability of the gland membrane be thus diminished, the slight vaso-dilation caused by the drug may be insufficient to cause a secretion, whereas a larger vaso-dilation on stimulating the chorda might overcome this resistance. Another possibility is that the quinine reaches a portion only of the alveoli, poisons these, and throws their capillaries and arterioles into dilation.

On stimulating the chorda the secretion may be derived from unpoisoned alveoli of which the blood vessels have not hitherto been in dilation.

The value of Langley's and Heidenhain's observation, that the secretory fibres of the chorda tympani recover, after nicotine poisoning, before the dilator fibres, is seriously impaired by a defective method of determining whether vaso-dilation did, or did not, occur. If we admit that the rate of flow of blood from the gland's vein is a criterion by which we can determine whether vaso-dilation has or has not occurred their conclusion is justified. But reflection shows that if vaso-dilation be slight the amount of water passing out into the secretion might so reduce the bulk of blood flowing through the gland as to mask entirely all effects of the increased flow due to vaso-dilation. In fact, the flow of blood from the vein would be a safe criterion of dilation, only if there were no escape of liquid through the capillary wall, a condition which manifestly does not here exist. Langley's and Heidenhain's conclusion that the secretory function recovers before the dilator is, hence, unjustified. The same criticism applies, also, to Heidenhain's observation that after the chorda tympani has been cut and allowed to degenerate for three or four days stimulation still causes an increase in the paralytic secretion, but no increase in blood-flow from the vein.

c. Evidence of the Osmotic Character of the Salivary Secretions which are Accompanied by Vaso-dilation.

wish now to summarize briefly those features of secretions, accompanied by vaso-dilation, which indicate that they are of an osmotic character.

(1) In structure the salivary glands have all the requirements of an elaborate osmotic mechanism They are, essentially, extraordinarily thin-walled bags, possessing an enormous surface, containing a mass of hydroscopic indiffusible substances. The outer surface of this bag is in intimate association with a mesh work of capillaries so coördinated by the nervous system as to permit an almost instantaneous flooding of the gland mem-

brane. Plainly here are all the requisites of a delicate osmotic mechanism adapted to the most rapid osmosis.

(2) Chorda secretion is closely dependent on blood supply. (Compare p. 342.) Heidenhain has shown that partial occlusion of the artery diminishes the rate of secretion (p. 88, Breslau Studien IV.)

(3) If the osmotic equivalent of the blood be increased by the injection of strong salt solutions the secretion is diminished or altogether inhibited.[54 38]

(4) If the osmotic equivalent of the blood be decreased by the injection of water the rate of secretion is increased.[38]

(5) The rate of secretion is increased, other things equal, by an increase in the rate of blood flow through the gland.[38 23]

(6) The rate of secretion diminishes when the hylogens are washed out of the gland. (Paralytic secretions, secretion after long stimulation.)[23]

(7) Substances may be absorbed with extraordinary rapidity when injected into the duct (nicotine, atropine).

(8) If the percentage of salts in the blood be increased the percentage of salts in the saliva increases also. If the percentage of salts in the blood be decreased, the percentage of salts in the saliva decreases also.[38 54 14]

(9) If the artery of the gland be clamped for 20–30 minutes, and the blood thus completely cut off from the gland, on readmitting the blood a vaso-dilation ensues, so that the blood rushes red from the gland veins, and this vaso-dilation is accompanied by a spontaneous secretion. Stimulation of the chorda in no way alters this secretion during the first minute, nor until the dilation has somewhat diminished. This spontaneous secretion is a close duplicate of that observed by Levy in the secretion of sweat. [Experiment V (a).]

Although this spontaneous secretion might, perhaps, be explained by supposing that a direct stimulation of nerve-end or cell by the oxygen has taken place, it seems more probable to me to class it with the spontaneous secretion of sweat in the horse, following section of the cervical sympathetic, and to refer it to the direct effect of vaso-dilation.

f. Conclusion. The Physiology of Salivary Secretion.

If the sympathetic salivary secretion shall be found to be due to the action of contractile tissue, and if the criticisms of the objections to considering the salivary secretion, coincident with vascular dilation, an osmosis, be sustained by subsequent work, the following conclusions concerning the physiology of this secretion may be drawn.

The salivary glands may be caused to secrete, either by the action of contractile tissue under control of the sympathetic nerve or by osmosis under control of the vaso-dilator nerve. Probably in normal secretion both of these nerves come into play, but of this evidence is as yet lacking.

Drugs, or other reagents, may arouse secretion by action on either or both of these mechanisms. I would suggest that secretion following strychnine injection, camphor, pikrotoxin, physostigmin (after division of the chorda) are due to the contractions of the contractile tissue. All of these drugs stimulate the nerve centers and cause a pronounced vaso-constriction. On the other hand, pilocarpine, nicotine, muscarine, curare and chloral hydrate, or other drugs with a similar action on the vascular system, probably cause secretion partly by vaso-dilation and partly by increasing the permeability of the gland membranes. Such drugs work through an osmotic mechanism. A third class of drugs, such as quinine, atropine, hydrochloric acid or sodium carbonate may produce vaso-dilation, but probably act, also, on the gland cells in such manner as to diminish their permeability. Most of the work which has hitherto been done upon the action of drugs on salivary secretion needs to be repeated with the possibility in mind that the chorda and sympathetic induce secretion in these different ways.

The osmotic mechanism of secretion in the salivary glands is probably dependent on the condition of the gland and capillary membranes, upon the composition of the blood, upon the rate of flow of the blood and the character and amount of hylogens present within the gland. The evidence that the course of osmosis is controlled by the action of nerves directly on the gland

cells is open to serious criticism. That chorda salivary secretion can ensue without vaso-dilation may be seriously doubted, not only for the reasons already stated, but because in the pancreas there is good reason to believe that secretion can not take place without vaso-dilation. (See p. 361.)

V. SOME OTHER SECRETIONS.

The submaxillary gland, considered in the foregoing pages, may be taken as a type of all the salivary glands, as each possesses a dilator secretory nerve, and a constrictor, sympathetic secretory nerve. I wish now to consider some other secretion in the light of the conclusions derived from the physiology of the submaxillary.

a. The Physiology of Sweat Secretion.

There is reason to believe that the mammalian sweat glands also have a double mechanism of secretion, a muscular and an osmotic. These glands are surrounded by a sheath of muscle fibres lying, like those of the skin glands of amphibia, between the cells and the basement membrane. From the observations of Ranvier, Joseph and others, who have shown that upon stimulation of the sciatic this muscle contracts, there can be little doubt that a secretion may thus be formed. Probably sweat secretions ensuing coincident with vaso-constriction, upon the injection of strychnine, upon stimulation of the sciatic in the amputated limb or after compression of the blood vessels is due to this mechanism.

On the other hand, certain secretions of sweat are too copious to be due to muscular constriction of the gland. That those secretions probably fall under the second, or osmotic, mechanism is shown by the following facts:

(1) The coincidence of vaso-dilation and sweat secretion. Most sweat secretions are normally accompanied by vaso-dilation. If the cervical sympathetic of the horse be severed, strong hyperæmia and sweating occurs on the side of the neck the nerve governs. This sweating ensuing after nerve division

can hardly be explained, I think, on the basis of secretory cell activity.

(2) Pilocarpine, which does not cause contraction of the muscular sheath, causes a profuse secretion.

(3) The vaso-motor and secretory fibres in the cat follow the same paths.

(4) Pilocarpine causes sweat secretions fourteen days after nerve degeneration.

(5) If the blood supply be cut off, on readmitting the blood after 30 minutes, a spontaneous secretion occurrs.[44] The similar secretion in the submaxillary is invariably accompanied by vaso-dilation.

(6) Increasing the capillary blood pressure or drinking large quantities of water increases secretion.

The facts, as far as they go, are the same as those observed in the cerebral salivary secretions and pancreatic secretion They justify us, I believe, in classing all three secretions in the same category. That these sweat secretions are of an osmotic character would thus be indicated. That other sweat secretions are due to muscle there can be little doubt.

b. The Secretion of the Pancreas.

Secretion of the pancreas is normally accompanied by vaso-dilation. In its relation to atropine, its increased content of organic bodies coincident with an increased rate of flow, and in taking place after compression of the aorta, pancreatic secretion resembles the submaxillary secretion on stimulation of the chorda tympani. There is reason to believe, however, that the pancreas cannot secrete unless the blood vessels dilate. Thus the means employed by Pawlow,[57] Mett[53] and Kudrewetsky[32] to give the vagi a secretory function are just the means used by Bowditch, Luchsinger and others[56] to give the sciatic and other mixed dilator and constrictor nerves a dilator action. These authors either cut the vagi and splanchnics, and allowed them to degenerate three or four days, or else they stimulated them with rythmic induction shocks, at the rate of one per second after division of the cervical cord. There are two possible ex-

planations of the fact that stimulation of the normal nerve with the cord undivided causes no secretion. Either the nerve carries inhibitory secretory as well as secretory fibres, or stimulation of the nerve is unable to cause a secretion without vaso-dilation. The first alternative Heidenhain has particularly combatted in the case of the submaxillary, and it appears to me lacking all proper experimental basis. The second alternative is probably the true explanation, for the reason that stimulation of the *normal* nerve below the cardiac branches causes no alteration in blood pressure, and for the reason that the treatment to which the nerve is subjected is calculated to give it a dilator action. If this be true the pancreas would appear fundamentally different from the salivary glands, unless, as I have endeavored to show, the latter are, also, in reality, unable to secrete on stimulation of the chorda or other cerebral nerve, unless vaso-dilation ensues.

Further evidence of the dependence of pancreatic secretion on vaso-dilation is furnished by the action of pilocarpine, chloral hydrate[19] and curare, drugs which cause vaso-dilation and secretion, and by strychnine,[19] or digitalis, drugs which cause vaso-constriction and inhibit secretion. Heidenhain,[23] also, has observed a close correspondence between vaso-dilation and secretion, and between vaso-constriction and the cessation of secretion. This parallelism between vaso-dilation and secretion can not be accidental. It indicates, I believe, that the dilation is the cause of the secretion, other things being normal.

VI. GENERAL CONCLUSION.

We have now considered the evidences of the existence of secretory nerves, and the reasons for believing that secretion is a function of the gland cells. While readily admitting the possibilities that secretion may in certain instances be a function of the gland cell, controlled by the action on it of secretory nerve fibres, we have seen reason to believe that certainly many so-called secretions are due not to the gland cell, but to the action of contractile tissue either within or about the gland. Among

such secretions are the salivary secretions following stimulation of the sympathetic, certain secretions of sweat, the secretion of the cephalopod salivary glands and of the skin glands of amphibia.

Whether those secretions which are normally accompanied by vaso-dilation, such, for instance, as the salivary secretions following stimulation of the cerebral nerves and the secretions of the alimentary tract and its appendages, are governed by nerves acting directly on the gland cells, or indirectly through the vascular system, cannot with certainty be said. But I believe it has been shown in the present paper that the evidence which has hitherto been offered that such secretions are controlled by nerve action on the gland cell is open to serious criticism. The remarkable parallelism between the hypothetical secretory and vaso-dilator fibres, the close dependence of such secretions on the vascular system, the general features of such secretions and the structure of glands, all indicate, I believe, that osmosis is the essential cause of these secretions, and that they are controlled by the action of nerves on the vascular system. No one would deny that the course of these secretions is modified by the condition of the gland or capillary wall, and that that condition is easily affected by drugs, but that nerve action directly affects that condition, I do not believe the evidence entitles us to say.

Probably the study of these secretions from the standpoint of osmosis will bring to light facts difficult to reconcile with our present knowledge of osmosis. But while our knowledge of the latter process through membranes undergoing chemical change, such as gland membranes, remains in its present fragmentary state, I do not believe that we are justified in assuming a special sort of secretory activity on the part of the gland, or capillary cell, unless the facts are certainly irreconcilable with any other hypothesis.

In short, while fully admitting the possibility that nerves may act on gland cells, in some way affecting osmosis through them, it appears to me that, in the present state of our knowledge of secretion, the assumption of a particular secretory function of

cells, and of special secretory nerves, is unwarranted, unnecessary, and, in certain particular cases, opposed to the phenomena of the secretion itself.

SUMMARY OF RESULTS.

(1) The sympathetic nerve induces salivary secretion by acting on contractile tissue in the glands and thus causing a compression of ducts and alveoli.

(2) The chorda tympani, or other dilator salivary, secretory nerve probably causes secretion by its dilator action on the blood vessels, thus increasing osmosis.

(3) The evidence that the chorda tympani acts on the gland cells is open to serious objections, as follows :

(*a*) Atropine probably acts directly on the gland cells and capillary endothelium, diminishing their permeability.

(*b*) The post-mortem chorda salivary secretion is possibly due to a back flow of blood from the veins owing to a dilation of the capillaries.

(*c*) The increased content of organic matter in a secretion coincident with an increased rate of secretion is of little value as evidence of secretory nerves, because (1) saliva is generally not a true solution, and (2) a weak stimulus probably arouses but a portion of the gland.

(*d*) The evidence derived from the action of nicotine and the degenerated chorda tympani that secretion may ensue on stimulation of the chorda without vaso-dilation is of doubtful value, because of an erroneous method of determining that vaso-dilation had not occurred.

(4) The sweat glands and the amphibian skin glands, like the salivary glands, receive a double nerve supply and probably possess a double mechanism of secretion, *i. e.*, a muscular and an osmotic.

(5) Whether secretory nerves exist or whether secretion is ever a function of the gland cell must be considered at present an open question.

(6) The thoracic lymph flow in dogs reacts to nerve stimula-

tion and drugs very similar to pancreatic secretion. It is increased by rhythmical stimulation of the vagi after division of the cervical cord and by pilocarpine and chloral hydrate, and decreased by atropine.

COLUMBIA UNIVERSITY, April, 1898.

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SOME PASSAMAQUODDY DOCUMENTS.

J. DYNFLEY PRINCE.

(Read April 25, 1898.)

THE Passamaquoddy Indians of Maine are members of the *Wabanaki* or northeastern group of the great Algonkin family which in earlier times occupied territory extending from James' Bay on the north to the Carolinas on the south. The *Wabanaki* tribes which still exist are (1) the St. Francis Indians, of Canada, who are at present a small sept of mixed race resident on the St. Francis river, near Quebec. These people, who call themselves by the generic name *Abnaki* or *Wabanaki*,[1] are composed of *Wabanakis* of various tribes from New Hampshire and Massachusetts, of Sagadahoks, and of Norridgewoks,[2] from Maine. (2) The Penobscot Indians of Maine are very closely allied both in race and language to the St. Francis tribe. (3) The Passamaquoddies[3] of Maine are practically identical with (4) the Maliseets (Milicetes) of New Brunswick. (5) Finally, the Micmacs of Nova Scotia and New Brunswick constitute the easternmost branch of the *Wabanaki*.

The Passamaquoddies, like many other Indian tribes, have an extensive oral literature, consisting of historical, mythological and legal traditions, as well as many songs and recitations. A great part of this material is preserved by means of a mnemonic system of wampum shells arranged on strings in such a manner as to suggest to the mind of the reciter certain sentences of a tale already committed to memory.[4]

In 1887, during a visit to Bar Harbor, Me., I obtained from Mr. Louis Mitchell, a Passamaquoddy Indian, who was at that time Indian member of the Maine Legislature, some selections from this oral material which he had committed to writing. Undoubtedly, the most important of these, both from an his-

torical and ethnological point of view, are the so-called Wampum Records, which embody a detailed description of various ancient rites and ceremonies, peculiar not only to the Passamaquoddies, but to all the northern Algonkin clans (*Wabanaki*), who, after a long period of internal strife, seem to have formed a close offensive and defensive alliance. These records I have published in the *Proceedings of the American Philosophical Society*, XXXVI, pp. 479–495. Besides the Wampum Records, I have a number of other documents, the most important of which is an outline of the *Wabanaki* history previous to the establishment of the inter-tribal treaty of peace between the *Wabanaki* clans and the foundation of the common *modus vivendi* set forth in the Wampum Laws.

I have ventured in the following pages to reproduce this historical sketch, which has, at least, the merit of being purely native, and, as a specimen of the Passamaquoddy poetic genius, I have added, in both Indian and English, part of a characteristic love-song.

The original text of the Indian history was not included in the manuscript which I received. It is necessary to remark that, as Mr. Mitchell's translations were written in what may be termed Indian-English, I have been compelled to rearrange his versions into our current vernacular. His Indian text, both in the song in the present paper and throughout the Wampum Records, is written syllabically without any attempt to divide the sentence into words, so that it is extremely difficult to edit the Passamaquoddy original with even approximate correctness.

Wabanaki History Previous to the Establishment of the Wampum Laws.

In former days the *Wabanaki* nation, the Indians called *Meguyik*[5] or Mohawks and other members of the Iroquoian Six Nations[6] were want to wage bloody and unceasing war with one another. The *Wabanaki* nation consisted of five tribes, *e. g.*, Passamaquoddies, Penobscots,[7] Micmacs, Maliseets[8] and the tribe (now extinct) which formerly inhabited the banks of the Kennebec river.[9] The bitterest foes of the *Wabanaki* were un-

doubtedly the *Meguyiks* or Mohawks, who, on the slightest provocation, would send bands to harry them and destroy their crops. The Mohawks invariably treated their prisoners with the most merciless severity, showing no pity even to the women and children. A favorite torture which they frequently practiced was to build a large fire of hemlock coals, into the flames of which they drove their captives, compelling them to walk back and forth over the glowing coals until relieved by death. No case is on record where a brave of the *Wabanaki* nation succumbed to the pain. Their warriors would always pace the fiery path with undaunted resolution and without uttering a sound until nature put an end to their agony. Tortures of this sort were practiced by all the tribes, but the Mohawks exceeded the others in cruelty.[10]

The cause of the strife was an hereditary dispute about hunting grounds. Besides the enmity which they nourished in common against the Six Nations, the *Wabanaki* had also internal disputes. Thus, the Penobscots were at feud with the Maliseets and the Micmacs with the Passamaquoddies.

The first war between the last mentioned tribes was brought about by the quarrel of two boys, sons of chiefs. On this occasion the Passamaquoddies were on a friendly visit to the Micmacs, during which the sons of the Passamaquoddy and Micmac chiefs went shooting together. They both shot at a white sable, killing the animal by their joint effort, but each lad claimed it as his game. Finally, the Passamaquoddy boy, becoming enraged, killed the son of the Micmac chief. The latter on hearing of the murder could think only of vengeance and positively refused to listen to the Passamaquoddy chief's attempt at conciliation. The latter even offered the life of his own son who had been guilty of the murder, but all to no purpose. In consequence of this unfortunate occurrence the celebrated "great war" was then declared which lasted many years.

The Micmacs, although more numerous than their enemies, were inferior warriors, so that the victory was always (*sic*) won by the Passamaquoddies. So great was the hostile spirit that the two tribes fought whenever they met, paying no heed to the

time of year. On one occasion, the Passamaquoddies went to *Tlancowatik*, thirty miles west of St. John, N. B., with a small party consisting principally of women and children with the chief and a few braves. At this place they met a number of Micmacs on their way to Passamaquoddy Bay. The Micmac chief being a lover of fair play ordered his men to land on an island to await the coming of a messenger. The other chief sent word that on the following day "the boys would come out to play." As the Passamaquoddy chief had very few men able to bear arms, he made the women attire themselves like warriors, so that from a distance they might be mistaken for men and directed them to play on the beach shouting and laughing as if entirely fearless. The Micmac chief, deceived by this stratagem and being afraid, summoned his braves to council and setting forth the disasters which had been caused by the long war advised a treaty of peace. This proposition was made to the Passamaquoddies who, wearied by the perpetual state of unrest, gladly acceded to the request. A general council was accordingly called, by which it was decided that "as long as the sun rises and sets, as long as the great lakes send their waters to the sea, so long should peace reign over the two tribes."

The usual ceremonies for making peace were then observed, as follows: (1) a marriage was contracted between a brave of the challenging people and a maiden of the challenged people. This was regarded as a type of perpetual future good will. (2) A feast lasting two months was celebrated nightly and (3) games of ball, canoe and foot races and other sports were carried on. After such ceremonies were over no breach of a treaty is on record, not even a single murder.

After the great Micmac war was ended, the Passamaquoddies lived at peace except for occasional raids of Mohawks, but the latter finally received a blow from which they never recovered, the details of which are as follows: It was the custom of the Mohawks to make night attacks and at one time, when the Passamaquoddies were at the head of Passamaquoddy Bay,[11] the Mohawks approached the camp, which was called Quenasquam-

cook,[12] with the purpose of utterly destroying it. On this occasion, however, they were seen by a Passamaquoddy brave whose people lay in ambush for them. It was the custom of chiefs to wear medallions of white wampum shells which were visible at a long distance, particularly in the moonlight. Picking out in this way the person of the Mohawk chief whose name was *Lox*, "panther," [13] the watching braves shot him first, owing to which calamity the Mohawks were thrown into confusion and fled. The Passamaquoddies followed them as soon as day broke, but the tracks were so scattered that they could not find the refugees. It was ascertained afterwards that the Mohawks had quarreled among themselves, one party being in favor of making peace with the enemy, while another faction was strongly opposed to such a measure. The discussion of the question ended in a fierce combat. This was the final blow to the Mohawk cause, so that the nation ever afterward sought to be at peace with the Passamaquoddies.

After this battle the Passamaquoddies were never again molested, but the Penobscot tribe was still at war with the Maliseets and Mohawks and, in fact, were nearly destroyed three times by their ruthless foes. A most interesting legend of this Mohawk war is the account of the miraculous revelation to the Penobscots by *Wenagameswook* or fairies of the approach of a large body of Mohawks. Two Penobscots were coming down the Penobscot river from their winter hunting, when they spied a newly made canoe paddled by what seemed to be two small boys who, pursue as they would, always kept at an even distance ahead of them. Finally, the supposed children stopped and called out to the wondering Indians "*Nowut Kemaganck Meguyik*" "At *Nowut Kemaganck* there are Mohawks." As the hunters had noticed some chips floating down the stream, they believed the report at once. The Mohawks had been making rafts with which to float down the river in order to destroy the Penobscot tribe. As soon as the hunters reached Oldtown they told their curious tale, which was immediately credited by the old men, who straightway prepared for war. The fairies, according to their belief, always either appeared in person or

carved a warning on rocks before a danger which threatened the tribe. Greatly excited, the Penobscots despatched scouts in all directions, so that when the Mohawks arrived, they found the warriors perfectly prepared awaiting them behind a brush-wood breast-work (*lisignigen*).

No damage was done at that time, but on another occasion the Mohawks completely defeated the Penobscots, saving only one man as a guide to the St. John's river (*Wulastuk*).[14] Constructing rafts there, they aimed to float down strean for the purpose of destroying the village of Maliseets (*Wulastukuk*). The Penobscot guide told them that there was no falls or rapids before them, knowing full well all the time that on this river is the great fall of Chikchenikbik of nearly 100 feet, the roar of whose torrent is perfectly inaudible to the traveler until he is within a few yards of it. The Mohawks, trusting to their guide, were all sleeping on their rafts, when the Penobscot, suddenly jumping overboard, swam ashore and left his 600 sleeping foes to be carried over the falls. Not a man escaped to tell the tale except the Penobscot guide.

The Mohawks, discouraged by their repeated failures, decided to make a treaty of peace among all the nations, apportioning the disputed hunting grounds as follows: To the Penobscots, the Penobscot river and its tributaries; to the Maliseets the St. John's river and its tributaries; to the Passamaquoddies, the St. Croix river[15] and its territory, and to the Micmacs their own streams. The *Wabanaki* lived ever after as one nation, undisturbed by internal strife and keeping the Mohawks and Six Nations at peace with them by presenting a united front. This is the origin of the Wampum Laws which were the product of the union of the tribes.

The following song is the plaint of the youthful Indian lover who sings to his fair one before going away to his winter hunting in the autumn when the leaves are *red*. He promises to return to her embraces in the Spring when the *green* foliage has begun to bud. The song has in the original four sense-stanzas. A refrain precedes the first, second and fourth and is repeated for the last time after the fourth verse. In Mitchell's MS. no

translation is given of the fourth stanza, so that I have omitted it and the final refrain in the present paper.

Passamaquoddy Love Song.

Refrain.

Anigowanotenoo! Boski k'tlabin elmi nelemwik elmi papkeyik; boski k'tlabin, Anigowanotenoo!	Anigowanotenoo! Oft on a lonely day thou look'st on the beautiful river and down the shining stream. Oft thou lookest, Anigowanotenoo!
1. Neket m'pesel etli-nemiot-yikw. Etuchi w'linakw-ben se-bayi sibook; etuchi w'li baquas-keten. K' machtena nolithasiben; mechinoltena keppitham'l, Anigo-wanotenoo!	When last I saw thee, how beautiful that fair stream looked, how lovely was the silver moon. Thou knowest how happy we were. Ah, since that night I think of thee always, Anigowan-otenoo!

Refrain.

Boski k'tlabin elmi nelemwik naga elmi papkeyik Anigowanote-noo.	Of't on a lonely day thou look'st on the beautiful river and down the shining stream, Anigo-wanotenoo?
2. Negetlo he eli-alnisookme-kwben sebayi guspenik etuchi we-lanakw-sititben wuchowek he eli-machip klamisken mipisel, Anigo-wanotenoo!	When we paddled the canoe together on that beautiful lake how fair the mountains looked and how we watched the red leaves whirl in the gentle breeze, Anigowanotenoo!
3. Anigowanotenoo, nittloch apch eli-alnisooknukw tan etuch apachyaie; tanetch etuch boski p'kesik mipisel yut pemden nit k'tlaskooyin.	Anigowanotenoo, we will go once more in a canoe and watch the beautiful green leaves on the mountain.

Refrain.

Boski k'tlabin elmi nelemwik elmi papkeyik, Anigowanotenoo.	Of't on a lonely day thou lookest on the beautiful river and down the shining stream, Anigo-wanotenoo!

EXPLANATORY NOTES.

[1] *Wabanaki* means "inhabitants of the East or dawn country" from *waban* "daybreak" and *aki* "land, territory." The latter is a suffix used in composition for "land, region" (see Brinton, The Lenape and their Legends, p. 191).

[2] Norridgewock or Norridgewalk is on the Kennebec and not as Gatschet states on the middle Penobscot (*Nat. Geogr. Mag.*, VIII, p. 23). Its original name was *Nanrantsouack*, which may have meant "stretch of still water," although this is not certain. The settlement was the home of the nucleus of the present St. Francis clan, where Father Rasle, the author of the Abnaki dictionary, first established himself in 1689 (see Pickering's edition of Rasle's work in *Amer. Acad. Sci. and Arts Mem.*, New series, 1833, Vol. I, p. 372). The tradition of the present Abnakis of Canada asserts that their ancestors came from Maine and New Hampshire.

[3] The Indian form of the name Passamaquoddy is *Peskatumagatiek* "those belonging to the place abounding in pollock-fish" (*peskatum*); cf. Gatschet, *l. c.*, p. 23.

[4] See Prince, *Proc. Amer. Philos. Soc.*, XXXI, p. 480.

[5] The real Mohawks called themselves *Caniengas.* When first known they were living on the south side of the Mohawk river between Canajoharie and Schoharie creeks in New York Province. Being loyalists, they removed to Canada with Brant at the time of the American Revolution (Hale, the Iroquois Book of Rites, p. 34). It is probable that Mitchell means here by Mohawks (*Mcguyik*) not only the *Caniengas*, but also the Canadian Iroquois. The whole Iroquois race is called in the St. Francis language *Magua*, and indeed the term Mohawk which is a corruption of the word *Magua* was used in England in much the same way.

[6] Originally Five Nations, *e. g.*, Onondagas, Mohawks, Oneidas, Senecas and Cayugas who called themselves in the Iroquoian dialect *Hotinonsionni* (Prince, *l. c.*, p. 438). The Tuscaroras came into the league later. The Iroquoian name for the completed federation was *Kanonsionni* "the league of the united households" (Hale, *l. c.*, pp. 10, 171).

[7] The original form of the name Penobscot is *Panawampskik* (St. Francis *Panapskak*) "where the steep rocks are."

[8] The Maliseets (Milicetes) who speak practically the same language as the Passamaquoddies are called Etchemins by the Micmacs.

[9] Undoubtedly the Norridgewoks mentioned notes. Kenebec is probably a slight alteration of *Kinebek* "deep river."

[10] In connection with this undoubtedly biassed statement, cf. Hale, *l. c.*, p. 83, ff. on the Iroquois character. There is no reason to believe that the members of this much-maligned race were any more barbarous in the treatment of their captives than their hereditary Algonkin foes.

[11] See note 3 and Gatschet, *l. c.*

[12] *Quenasquamcook* "at the gravel beach of the pointed end" (cf. Gatschet, *l. c.*, p. 22).

[13] Delaware, *quenischquney*, literally "long tailed" (Brinton, Lenape Eng. Dictionary, p. 121). Ojibwa *peshew;* Micmac *utkogwech.*

[14] *Wulastuk* "the good river"=Aroostook, *e. g.*, the St. John river. *Wulastukuk* is a locality "at or near the St. John river." The Micmacs call St. John *Menawges* "the place where dead seals are collected."

[15] Called *Skutik* in Passamaquoddy (according to Mitchell) from *squt* "fire;" "at the fire," owing to the custom of spearing salmon by torchlight. It is much more likely that the name is an allusion to the burnt lands or clearings on the banks of the river or on Schoodic lake. Tradition asserts that large forest fires took place here about 1675.

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THE PHYLOGENETIC SIGNIFICANCE OF CERTAIN PROTOZOAN NUCLEI.

GARY N. CALKINS.

(Read April 11, 1898.)

[PLATE XXXV.]

THE nucleus is often looked upon as a more or less well-defined morphological element of the cell, possessing in its various phases a common type of structure and composed in all cases of similar substances. A comparison of cells in various tissues whether vertebrate or invertebrate, plant or animal, shows that in the majority of cases the nuclei are so similar that, with slight variations, a description of one answers for a description of all. In resting phases the similarity is shown in the distribution of chromatin, linin, and in the nucleoli, while the nuclear membrane is usually present. In active phases metazoan nuclei as a rule, pass through the same stages of spirem-formation, loss of membrane, chromosome-formation, and various processes of re-formation. The differences between such nuclei being confined mainly to variations in number of chromosomes, in arrangement in the nuclear plate, and in the mode of division.

The nuclear type being so constant in higher animals we must look to the lower animals—that is, to the Protozoa—to find not only the prototype, but any transitional forms leading up to the highest types, bearing in mind, however, that notwithstanding the constancy of type manifested in the nuclear forms and mitotic processes of the latter, individual differences may have arisen and mitotic processes may have developed in quite diverse ways. In the present paper it is my object to bring together a few facts, some of which are new, showing how in the Protozoa, the nucleus of the type found in Metazoa

may have arisen from simpler forms, and how in its mitotic phenomena it passes through stages represented by permanent nuclei of lower Protozoa.

In the first comparison of metazoan with protozoan nuclei we are at once beset with difficulties. Protozoan nuclei vary so widely among themselves that, save for the same class of organisms, a description of one nucleus would not correspond at all to that of another. Some resemble the ordinary type of nucleus in the Metazoa, others are so different from this type that they can scarcely be compared. In general the nuclei of the Protozoa are much simpler in structure than those of Metazoa. Chromatin is present in all cases but other parts which are usually found in nuclei of the Metazoa are frequently missing, *e. g.* the linin, the nuclear membrane or the nucleolus. On the other hand, bodies are occasionally found within the nucleus of Protozoa which are absent altogether or present in some other form, in Metazoa; such for example are the significant centrosome-like bodies found in *Euglena* and allied forms.

There are so many different types of nuclei in the various classes and orders of the Protozoa that it should be possible to select a chain of forms connecting the simplest known type with the highest. Such a sequence may be sought for in the structure of the resting nucleus or in the method of division. An ideal sequence would result if the two lines could be developed simultaneously, but this is extremely difficult as a nucleus may be high in the series of nuclear structures and low in the matter of mitotic division. The nuclei of *Actinosphærium*, *Actinophrys* and *Noctiluca* offer a striking example of this fact, the two former resembling the structure of the metazoan type more closely than the latter, while in mitosis the latter is much nearer the metazoan type than are the former. In questions of phylogeny however, morphological characters are usually of more importance than physiological characters and this must be kept in mind in the present discussion. A number of authors have built up theories of phylogeny on the method by which the nuclei of Protozoa divide, and the obvious result is a series of mitoses which satisfy to a certain extent the requirements in

such a scale, but the series applies only to the nuclei during division while the nuclei at rest are quite different, and the cell-bodies to which the nuclei belong, often represent widely separate classes of animals. A phylogeny based upon such a foundation must necessarily be weak, for it is perfectly possible that various classes of Protozoa may develop mitotic modifications quite independently of each other and yet along the same lines.

In view of the fact that the nuclei of the Protozoa show such wide differences it is not surprising that some forms should possess no structures which can be accurately defined as nuclei. Indeed, if the nucleus be regarded merely from a morphological standpoint it is quite easy to conceive of cells which possess no nuclei (Haeckel's Monera, in part) and to imagine groups of cells intermediate between such forms and those in which a definite morphological nucleus can be made out. These intermediate forms are the subject of the present paper.

The observations were made on various Protozoa including simple flagellates, dinoflagellates, rhizopods, heliozoa, ciliates, suctoria and *Noctiluca*. The material was fixed with sublimate acetic (5 per cent. acetic), picro acetic, Hermann's fluid, and saturated sublimate. The stains used were mainly iron hæmatoxylin with orange or Congo red, and the Flemming triple. The nuclei were studied from thin sections or from total preparations, sections giving the best results.

The So-called "Distributed Nucleus."

A number of forms which Haeckel included in his enucleate Protista, have subsequently, by the use of better optical instruments and improved technique, been found to contain minute particles of chromatin which are distributed without definite order throughout the cell. Such types have been called distributed nuclei. Occasional instances of this type of nucleus have been found in nearly every group of Protozoa. In the Ciliata, Gruber ('84) found that *Chœnia teres* and *Trachelocerca phœnicopterus* possess no true nuclei but minute granules of chromatin distributed throughout the cell-substance. These granules, ac-

cording to Gruber, unite into a common body previous to division and are then halved.

The Ciliata are highly specialized Protozoa and it is probable that, among them, the primitive distributed nucleus is very uncommon ; we should expect to find this condition in the simpler and less differentiated forms like the flagellates or the lowest plants. In the latter, especially the bacteria and the closely allied Cyanophyceæ, Bütschli ('90 and '96), confirmed by Zacharias ('90), described cells possessing a distinct protoplasmic structure enclosing numerous granules which he found to be chromatin. Bütschli regards these cells as nuclei with only a fine layer of protoplasm around the outside. The chromatin is laid down on what appears to be the cytoplasmic reticulum but which according to his view, would be linin. However this view may be in regard to the bacteria it cannot hold for cases of distributed nuclei among the Protozoa.

A flagellate belonging to the genus *Tetramitus* possesses a nucleus of the same distributed type. The protoplasmic structure of this flagellate is strikingly similar to Bütschli's figures and photographs of *Chromatium* and other bacteria. The periphery is characterized by a distinct alveolar layer consisting of vacuoles of regular size and arrangement, and the walls which bound them. The central portion is made up of alveoli of various sizes and is much looser in texture than the outer layer (Plate XXXV, Figs. 1–4). After fixation with Hermann's fluid and staining with Flemming's triple stain, this cytoplasmic structure appears yellowish or of an orange tone. In the endoplasm the substance of the alveoli appears to have run together at one point to form a more compact, denser aggregate which, with the stain used, appears homogeneous (Fig. 1 *A*). With the iron-hæmatoxylin the fused portion becomes more conspicuous although not more deeply stained than the cytoplasmic reticulum. It appears to be a coalescence of cytoplasmic microsomes. No inner structure could be made out, although in some cases a lighter area (Fig. 2) was faintly indicated in the center. In some individuals the body in question appears biscuit-shaped as though undergoing division (Fig. 4).

In addition to the above structure the cells of *Tetramitus* contain a number of comparatively large-sized granules which stain intensely with saffranin in the Flemming stain and black with the iron-hæmatoxylin. In division they are separated into two equal groups in the daughter-cells (Fig. 5). From the relation to stains and the general appearance during rest and division I have no hesitation whatever in comparing them with the granules of chromatin described by Bütschli in the case of *Chromatium* and *Bacterium termo.* The most frequent position of the granules is at the extremity of the cell opposite the flagella, where they form an aggregate of greater or less density, but in which the individual granules can be distinctly made out (Figs. 2, 4). There is reason to suppose that this close aggregation indicates the approach of division, for the culture was extremely active and the monads were increasing rapidly. Many individuals were found, however, in which the chromatin granules were distributed over all parts of the cell (Fig. 1). Aggregates were also found in the flagella-end of the cell (Fig. 3), although such cases were comparatively rare. In this connection it is a significant fact that division of the body begins at that end of the animal which holds the chromatin, in this case at the posterior end, although the majority of flagellates begin to divide at the flagellate end (Fig. 5).

This type of nucleus must be very primitive. It has no membrane and no linin unless the meshes of cytoplasm around the chromatin granules be called linin. Is there a nucleolus? Were it intra-nuclear the cytoplasmic body described above might be called a nucleolus on account of its staining reactions, but it is not intra-nuclear, and furthermore it appears to have a special function in the activity of the cell. Wherever the aggregate of chromatin granules may be found the cytoplasmic body is invariably in the near vicinity. It appears to divide before the group of chromatin is halved, and in the daughter-cells of a just-divided form the chromatin granules appear to surround the cytoplasmic body (Fig. 5.) It is certainly conceivable and in view of the phenomena in other allied Protozoa, almost probable that this cytoplasmic body exerts some influence upon the chromatin granules to attract them about itself at certain stages.

The Intermediate Type of Nucleus.

The "intermediate type" includes those nuclei which have either a faint nuclear membrane or none at all, and which persist in the form of spherical aggregates of chromatin granules about a central attractive body. The majority of the common autoflagellates possess nuclei of this type and a description of a few will suffice for all. The forms selected are *Microglena*, *Synura*, *Chilomonas*, *Trachelomonas* and *Euglena*.

Microglena punctifera (Pl. XXXV, Fig. 6). This minute form possesses two large chromatophores which occupy the greater part of the cell and which obscure the finer protoplasmic structure. There is a single flagellum attached at the end where the chromatophores come together. At this end also a small pigmented "eye-spot" can be made out (Fig. 6, E). The nucleus lies between the chromatophores in the center of the cell. It consists of a large number of chromatin granules surrounding a deeply-staining central body. The granules are loosely arranged, often forming an irregular outline and apparently are not bounded by a nuclear membrane.

Synura uvella (Pl. XXXV, Fig. 7). This beautiful colony-form is similar to *Microglena* in regard to nuclear structure, and, being larger, the details can be more readily made out. The monads are attached at a central point by their sharp ends, which form the lower extremity of the gelatinous mantle surrounding the protoplasmic body. The two equal-sized flagella arise from the outer end of the protoplasmic body and run nearly parallel through the outer mantle. Two large chromatophores occupy the greater part of the cell, each being curved like the half of an empty nut shell. The nucleus is enclosed in the space between the chromatophores. It is excentric in position, lying nearer the flagella end. Like the nucleus of *Microglena* it is made up of fine granules of chromatin disposed around a distinct central body. Here also the chromatin appears to be free from a bounding membrane, but in both of these forms the nuclei are distinctly outlined and well marked off from the surrounding cytoplasm while they invariably appear round in section.

Chilomonas cylindrica Ehg. (Plate XXXV, Figs. 8, 9, 10.) This very common flagellate is characterized by the buccal depression typical of the family Cryptomonidæ, by two equal flagella, by an œsophagus-tract in the endoplasm, by absence of chromatophores, and in most cases, by the absence of plastids. The absence of cytoplasmic intra-cellular substances makes *Chilomonas* particularly favorable for nuclear study as well as for the study of cytoplasmic structure. The nucleus is a conspicuous body in the lower half of the cell just below the middle line. It is always irregular in outline, the irregularity being due to prolongations of its substance, like pseudopodia, into the adjacent protoplasm. We again find the granular chromatin and the intra-nuclear deeply-staining body. In this case, there is, in all probability, no nuclear membrane, and from the various shapes of the nuclei in different individuals it is inferred that the chromatin granules may become more or less scattered, although remaining in the vicinity of the central body. During division of the cell the chromatin becomes closely aggregated around the central body which divides first, the chromatin granules, as in *Euglena*, separating later into two equal portions. Division here is not as complicated, however, as in *Euglena*, for the chromatin granules do not fuse into distinct rods or chromosomes as in the latter form (Figs. 9 and 10).

Trachelomonas. Several species of *Trachelomonas* were examined and in all cases the nuclei were of the same type as those already described. Among the most noteworthy were the nuclei of *T. lagenella*, *T. volvocina* and *T. hispida*. The simplest of these was found in *T. lagenella* (Fig. 11) where, as in *Chilomonas*, it consists of an irregular mass of chromatin granules surrounding a central body. No nuclear membrane was seen, although the protoplasmic structure was plainly apparent. Compared with the entire nucleus the central body in this case is quite small.

Two varieties of *T. hispida*, which for convenience I shall characterize as variety A, and variety B present two distinct phases of nuclear arrangement. The two forms differ in other respects; variety A is smaller, has no collar, has a compar-

atively thick shell and is provided with fine, needle-like spines (Fig. 12). Variety B has a collar bearing six distinct spikes on its outer margin. Its wall is comparatively thin and is provided with spines of thorn-like structure, *i. e.*, with broad bases and sharp points (Fig. 13). In both cases the protoplasm is characterized by great vacuoles in which lie a varying number of plastids. The nucleus in variety A resembles that of *T. lagenella* in having an irregular mass of chromatin granules. The edge of the mass is irregular and more or less "frayed out," leaving little doubt as to the absence of a nuclear membrane. The central body is comparatively small and is either round or elliptical in form. The nucleus of variety B, on the other hand, presents quite a different appearance. It is very regular in outline, the margins are smooth and even, and a delicate though distinct membrane encloses it. The central granule is large and conspicuous (Fig. 13).

In *Trachelomonas volvocina* the nucleus resembles that of *T. hispida* variety B. The cell is somewhat more compact however, the nucleus is smaller and the cytoplasm contains more plastids (Fig. 14).

Euglena viridis (Pl. XXXV, Figs. 17, 18, 19). *Euglena*, of the same family as the *Trachelomonads* has, perhaps, the most highly differentiated nucleus of the intermediate type. Blochmann ('94) and Keuten ('95) described this nucleus as a group of chromatin granules enclosed by a membrane, and surrounding a central body—the "nucleolus-centrosome." Each chromatin granule was described as a "*Stäbchen*" or rod-like element. Bütschli ('90) had described, in addition to the chromatin granules and central body, a more or less distinct linin network which was apparently overlooked by Keuten. This so-called linin substance is extremely difficult to see but in thin sections and with the use of oblique light can be made out as delicate fibrils running from granule to granule. This structure could not be seen in the shelled euglenoids (*Trachelomonas*), possibly because the nuclei were not so easy to study, being total preparations of shelled forms. In other monads, as for example *Chilomonas*, which are as easy to study in total preparations as

the thinnest of sections, no such fibrils could be made out, although an occasional microsome between the chromatin granules aroused the suspicion that the latter are laid down on the cytoplasmic reticulum, in which case the inter-chromatin cytoplasmic net might be called linin in view of the connection which has been established between cytoplasmic and nuclear networks in the Metazoa.

In *Euglena* the central granule apparently exerts an attractive force during division. The chromatin granules aggregated in the form of small rods—primitive chromosomes—are arranged about it radially. The entire nucleus is surrounded by a delicate though distinct membrane.

Although the structure of the nucleus of *Euglena* and its behavior during cell division have been carefully described by Keuten, the differences between the chromatin and the central body do not seem to have been sufficiently brought out. A carmine stain, for example, is not sufficient to distinguish chromatin from plasmosomes and from the results shown by the iron-hæmatoxylin stain the central body would appear to be chromatin. A very delicate differential result is obtained by the use of the Biondi-Ehrlich mixture of methyl green and acid fuchsine (Auerbach's formula). After this stain the central body is distinctly red and shows out in marked contrast to the green of the surrounding chromatin. From this reaction it follows that the chemical composition of the central body is different from that of chromatin, a result which brings this body even more closely in line with the attraction sphere of the higher forms.

At this point Schaudinn's observations on *Paramœba Eilhardi* are interesting and important. *Paramœba* is a rhizopod with flagellate swarm-spores. The spores resemble *Chilomonas* in general appearance, but a peculiar *Nebenkörper* is found in the former which is lacking in the latter. This body is outside the nucleus which, although it seems to have no nuclear membrane, is nevertheless distinctly marked off from the rest of the cytoplasm. During cell-activity the *Nebenkörper* assumes a dumb-bell shape and, when the ends are well separated but still

held together by a connecting rod of its own substance, the chromatin granules begin to migrate towards the center of the connecting rod and finally form a complete ring around it. The *Nebenkörper* is thus a central body similar to the central body of *Euglena* at a corresponding stage in division (cf. Figs. 17, 18, 19). After division of the chromatin the daughter-nuclei are reformed, but in each case the central body is left out in the cytoplasm. This phenomenon recalls the conditions in *Tetramitus* where a similar protoplasmic body acts in a very similar manner (cf. Figs. 1 to 5). The latter form is more primitive however, for the chromatin is not collected in a definite body—the nucleus—but is distributed throughout the cell and collects only during and for cell division.

Protozoan Nuclei of Typical Metazoan Structure.

A typical metazoan nucleus differs from the forms described above in having a distinct linin reticulum with chromatin laid down within it and forming a chromatin reticulum; often a more or less clearly differentiated nucleolus, and a nuclear membrane which usually disappears during mitosis. Many important differences are found when a comparison is made of the nuclei during division. In the majority of Metazoa there is a distinct spirem leading up to the formation of chromosomes in each case characteristic of the species; and distinct spindle-formation with centrosomes and spindle fibers. In the Protozoa a number of nuclei have been described which agree more or less closely with the requirements of such a nucleus. So far as the resting nucleus goes, *Actinosphærium* and the nuclei of some Sporozoa are similar to the Metazoan nuclei while the nuclei of *Actinophrys* and of *Euglypha* approach them in mitosis. The number of such cases, however, is very small and, when compared with the number showing the intermediate type, it is insignificant. In short, the vast majority of Protozoa excluding the infusoria possess various conditions of nuclei of the intermediate type.

Aberrant Types of Protozoan Nuclei.

While nuclei of the type described above in flagellates seem to lead by gradual stages into more complicated forms of the Metazoan type, other nuclei of the Protozoa seem to have developed along a divergent path and finally resemble only remotely the primitive forms on the one hand, and the higher forms on the other. These nuclei may be described as aberrant forms, although the number of such forms is probably greater than any other type among the Protozoa. In most cases, however, the structure can be traced back to more primitive forms of the "Intermediate type." A few examples which have come under my own observation must suffice. These are *Amœba proteus* among the Rhizopoda, *Ceratium* and *Peridinium* among the Dinoflagellata, *Noctiluca*, a Cystoflagellate, and *Stylonychia* among the Ciliata.

Amœba proteus (Fig. 16). The nucleus of this common rhizopod is of large size and of characteristic shape, resembling a biconcave disc. It is constant in shape and is bounded by a firm membrane which, together with the granular chromatin contents, can be easily made out while the animal is alive. The finer structure, however, is seen only in sections which, with these large forms, can be cut in any desired plane. The nucleus contains, in addition to the general ground substance, or nuclear sap, two kinds of staining substances one of which becomes intensely black with iron hæmatoxylin, while the other is gray. The more deeply staining substance is chromatin in the form of granules distributed throughout the nucleus; the other substance has the form of a disc lying in the center of the nucleus. Gruber ('83) calls this central mass the "nucleolus." In all of the specimens which I examined at this time the nucleus had the same structure and I am convinced that it is typical of *Amœba proteus*. The faintly staining central mass is perfectly homogeneous in structure and, although I have not seen it in division, I am confident that it is to be compared with the intranuclear body in the flagellates. In other species of *Amœba* the nucleus possesses an internal structure similar to the "nucleolus centrosome" of *Euglena* (Schaudinn, '94).

The nucleus of *Amœba proteus* can be regarded as similar to that of *Chilomonas* plus a nuclear membrane. There is no evidence of other intra-nuclear bodies such as linin, nucleoli, etc., nothing is present but chromatin and the central body. The tough nuclear membrane is possibly due to the peculiarly rough treatment which the nucleus undergoes in its cyclosis with the other endoplasmic substances.

Ceratium and Peridinium. Bütschli ('85) found a very curious structure in the dinoflagellate nucleus. Viewed from one side it appears to be of the regular reticulate type with local thickenings on the linin network; but, looked at from another side, the nucleus seems to be composed of rows of chromatin connected by delicate fibrils, the whole having a more or less honey-comb structure. Lauterborn ('95) confirmed Bütschli's description but added that the nucleus invariably contains one or two nucleoli, and that in division a peculiar rod-like body of "unknown significance" stretches across the division axis. Lauterborn is inclined to believe this structure homologous with the intra-nuclear body of *Euglena*. I have examined a number of Dinoflagellata from Puget Sound and Alaska including *Peridinium divergens*, *Dinophysis*, *Ceratium tripos*, *Ceratium fusus*, etc. and in all of them I have found the familiar intra-nuclear central body, differing however from the more frequent type in being sometimes single, sometimes double or multiple. (Figs. 21 *Peridinium divergens*, and 20 *Ceratium fusus*.) The peculiar rod-like or even lamellate structure of the chromatin is perhaps due to the fusion of chromatin granules, thus forming a permanent structure comparable to a spirem.

Noctiluca miliaris (Pl. XXXV, Figs. 22–26). Of very different structure is the nucleus of *Noctiluca miliaris*, a form possibly allied to the Dinoflagellata. The chromatin here is massed in from eight to eleven large reservoirs (Fig. 22), while the rest of the nucleus is filled with a granular substance of quite a different chemical composition. The whole is enclosed in a firm membrane. This nucleus would be difficult to understand were it not for the changes which the chromatin undergoes previous to division. The large reservoirs

disintegrate during the earlier stages of mitosis, forming smaller and smaller chromatin bodies, the final result being a great number of minute chromatin granules, which, as in *Chilomonas* or *Euglena*, are found distributed throughout the nucleus. The chromatin granules later unite to form distinct chromosomes. The formation of the chromosomes is entirely different from the account of the process given by Ishikawa ('94). The granules of chromatin unite in lines which are focussed at one side of the nucleus; these lines are the chromosomes, and they are subsequently divided through the agency of a complicated mitotic process in which centrospheres, central spindles and centrosomes play an important part.[1] In the early stage of division, when the chromatin is scattered throughout the cell in the form of minute chromatin granules, the nucleus of *Noctiluca* is obviously comparable with the nucleus of the intermediate type, while the vegetative condition can be conceived as due to the coalescence of the chromatin granules to form the large reservoirs. An essential difference in the nucleus of *Noctiluca*, however, is found in the absence of an intra-nuclear central body. The place of this important mitotic agent is taken by a large cytoplasmic sphere lying just outside the nuclear membrane. This sphere, during mitosis, plays the same part as the intra-nuclear body of the lower flagellates, but in a much more complicated way. While the chromatin granules are fusing to form the chromosomes the sphere divides to form a dumb-bell shaped body consisting of two daughter-spheres and connecting fibrous substance forming the "central spindle" (Fig. 23). The nucleus then bends around in the form of a U until it almost completely surrounds the central spindle. The chromosomes, focussed at the side of the nucleus which was turned towards the cytoplasmic sphere, now form a nearly continuous line or ring—the nuclear plate—around the central spindle (Fig. 23). At this period it can be found by sections that the nuclear

[1] For a description of the process of mitosis in *Noctiluca* see my paper, now in press, which will shortly appear in the *Journal of Morphology* on "Mitosis in *Noctiluca miliaris* and its Bearing on the Nuclear Relations of the Metazoa and the Protozoa."

membrane has disappeared from that portion of the nucleus between the chromosomes and the central spindle, and that the ends of the chromosomes are connected by distinct fibers—the mantle fibers—with centrosomes inside of the spheres (Figs. 24, 26). The chromosomes are then divided longitudinally beginning at the ends turned towards the central spindle, and one-half of each chromosome goes to form the daughter nuclei. The nuclei are reconstituted by the subsequent aggregation of the chromatin granules into the large reservoirs while the sphere in each case forms a definite body on the outside of the nucleus.

The staining reactions of the sphere in *Noctiluca* are the same as those of the intra-nuclear body in *Euglena* and *Chilomonas*, and the same as the cytoplasmic body in *Tetramitus*. During mitosis its history is remarkably similar to that of the *Nebenkörper* as described by Schaudinn ('96) in the case of *Paramœba*. I think therefore that there can be no doubt that the sphere in *Noctiluca*, the *Nebenkörper* in *Paramœba*, the cytoplasmic body in *Tetramitus*, and the intra-nuclear body of *Euglena*, *Chilomonas* and allied forms are all analogous structures and have the same physiological part to play in the activities of the cell.

The sphere in *Noctiluca* however possesses an element during division which has hitherto not been found in the corresponding intra-nuclear or cytoplasmic bodies described above. This element is a distinct centrosome which was first described for *Noctiluca* by Ishikawa and the presence of which I have demonstrated beyond question. In addition to the centrosomes furthermore there is a second set of fibers—the mantle-fibers—which connect the chromosomes with the centrosomes; nor have these been found in the simpler nuclei described above.

A number of the Protozoa agree with *Noctiluca* in the history of the chromatin, and several observers (Gruber, Hertwig, Brauer) have described the breaking down of large chromatin reservoirs or "nucleoli" as they have been erroneously called.

When we come to consider the nuclei of the Ciliata and the Suctoria we are met by a new difficulty. The nuclei are dimorphic, and the two forms differ as much in structure as they undoubtedly do in function. I have no new observations to

record on the structure of micro- and macronuclei, but it is possible that the facts given above may throw some light on their origin. A number of theories have been advanced to explain the origin of the micronucleus and the aberrant type of nuclei in the Infusoria in general. The usual form of theory is that the two types gradually arose by differentiation of a primitive bi-nucleated form, one of the nuclei becoming the micronucleus, the other the macronucleus (Bütschli, Lauterborn, etc). A serious objection to this theory is that the macronucleus is formed from one of the subdivisions of the micronucleus at each conjugation. Schaudinn suggested in his paper on *Paramœba* ('96) that the micronucleus and macronucleus of the Infusoria might have arisen from the *Nebenkörper* and nucleus respectively of forms like *Paramœba.* The possession of chromatin by the micronucleus is a serious obstacle to this theory, and yet the important part which the micronucleus plays in reproduction makes Schaudinn's suggestion valuable. If pure hypothesis be allowed it might be conceived that the micronucleus represents the cytoplasmic body of forms like *Tetramitus* and *Paramœba* plus a certain amount of chromatin while the macronucleus represents the nucleus with the remnants of chromatin minus the essential cytoplasmic body. The cytoplasmic body which appears to be essential to reproduction as shown by its universal presence, is found, in most cases, in only one of the nuclei, which persists, while the other degenerates.

General Conclusion.

Enough has been given above, I believe, to show that a type-form of nucleus can be found to which the nuclei of the various groups of Protozoa can be compared; divergent forms being explained as modifications of this type. Such a nucleus can be described in brief as consisting of two distinct substances, one of which acts as an "attraction" center, the other as chromatin in the form of granules. From this primitive type two lines seem to have developed, in one of which the attraction center remains outside of the nucleus (*Noctiluca*, *Paramœba*) while

in the other it is intra-nuclear (Euflagellata). The significance of the central granule as an attraction-center in the case of *Euglena* was early recognized by Bütschli ('87), Blockmann ('94), Keuten ('95), Lauterborn ('95) and others who saw in it a primitive centrosome. Hertwig more recently ('96) accepted the idea and explained the central body in *Euglena*, together with the large spheres in *Noctiluca* and the pole-plates found in various Protozoa as centrosomes of the type observed by himself in sea-urchin eggs after treatment with various salts. I have shown above, however, that the sphere in *Noctiluca*, the cytoplasmic body of *Tetramitus*, the *Nebenkörper* of *Paramœba* and the intra-nuclear body of *Chilomonas*, *Euglena* and allied forms are analogous structures and that they have the same physiological function to play in the activity of the cell. But it has also been shown that there is a true centrosome in the sphere of *Noctiluca*. The intra-nuclear body of *Euglena* therefore cannot be called a centrosome as the above-named observers have designated it, and cannot be compared with the centrosome of the Metazoa. It is comparable however with the cytoplasmic bodies of *Paramœba*, of *Noctiluca*, and therefore with the attraction-sphere of metazoan nuclei. Moreover, this element seems to arise in the simplest cases as a cytoplasmic structure and independently of chromatin or nucleus (*Tetramitus*). It appears therefore that Boveri's original conception of an independent cellular substance, the archoplasm, holds good in the case of the Protozoa. By considering the intra- or extra-nuclear body of Protozoa as archoplasm in the form of an attraction sphere, rather than as a centrosome, the various conflicting views in regard to these structures can be more or less brought together. By this view can be explained the origin and significance of the central spindle of the Metazoa (cf. *Centrodesmus* of Heidenhain); the origin of spindles without centrosomes in the higher plants (cf. Strasburger's *Kinoplasma*); and, to some extent, the various interpretations of the function, origin and fate of the centrosome. According to this view the centrosome is originally of minor importance, the sphere alone being functional as an attraction center. The centrosome appears to be of later origin, although

even in the higher tissues, as Flemming ('97) suggests, it is apparently not an organ of primary importance, but an organ which may be present in connection with cell-divisions, although not necessary for it.

There is also good evidence in this study of primitive nuclei to show that the common type offers an explanation of the changes which the constituents of the metazoan nucleus undergo during and preparatory to a division. Stated briefly this idea may be expressed as follows: (1) Before forming chromosomes the chromatin material of the metazoan nucleus is distributed in the nucleus in the form of minute chromatin granules, a stage representing the ancestral condition which in flagellates and lower plants is permanent; (2) the chromatin granules (Brauer, '93 *Ascaris*) secondarily fuse to form distinct bodies—the chromosomes—of definite form and number for each species; (3) the chromatin is in close connection with the kinetic center (centrosome or centrosphere plus central spindle), to accomplish this connection the nuclear membrane disappears (in most Protozoa the attraction sphere is inside the nuclear membrane and so in constant connection with the chromatin; in other forms of Protozoa where the attraction sphere is extra-nuclear as in *Noctiluca* and *Paramœba* the membrane disappears on the side of the nucleus nearest the sphere—*Noctiluca*—or there is no membrane at all—*Paramœba*). In all cases the chromatin at the time of division is collected around or between the spindle fibers, or in case of Protozoa, the attraction-sphere, possibly to ensure a more perfect division of this important substance.

Summary of Observations and Conclusions.

1. Metazoan and protozoan nuclei cannot be strictly homologized, but it can be shown that an intermediate series of forms connect them.

2. The nuclei of Protozoa are not all of the same type and in some forms they may possibly be absent. The simplest structure is the distributed nucleus, consisting of isolated chromatin granules scattered about the cell.

3. A higher type is shown by the "intermediate" nuclei, where the chromatin granules are massed together in a compact form with or without a nuclear membrane (most Euflagellates).

4. Typical nuclei of the metazoan type are uncommon among the Protozoa, but are occasionally found.

5. Nuclear differentiation in Protozoa is closely connected with an attraction-sphere or active agent in division. In nuclei of the distributed type this is an indefinite faintly staining cytoplasmic mass in the vicinity of which the scattered chromatin granules collect previous to division and about which they are grouped during division. In nuclei of the "intermediate" type the attraction-sphere is intra-nuclear, definite in form, deeply staining and active, and chromatin granules are massed about it either permanently (*Synura*, *Chilomonas*, *Englenoids*, etc.) or only during division (*Paramœba*), and with or without a nuclear membrane. In higher types of nuclei the attraction-sphere is no longer intra-nuclear, but this position of vantage is taken by the central spindle during division (*Noctiluca* and many Metazoa).

6. The intra-nuclear body of *Euglena* and other allied forms is equivalent to the attraction-sphere and not to the centrosome of the metazoa.

7. Chromosome-formation is first seen in the flagellates in the form of rods which arise by the union of the previously scattered chromatin granules. They form in typical though primitive metazoan manner in *Noctiluca* and *Euglypha* and all Metazoa pass through these stages in preparing for mitosis.

Columbia University, April, 1898.

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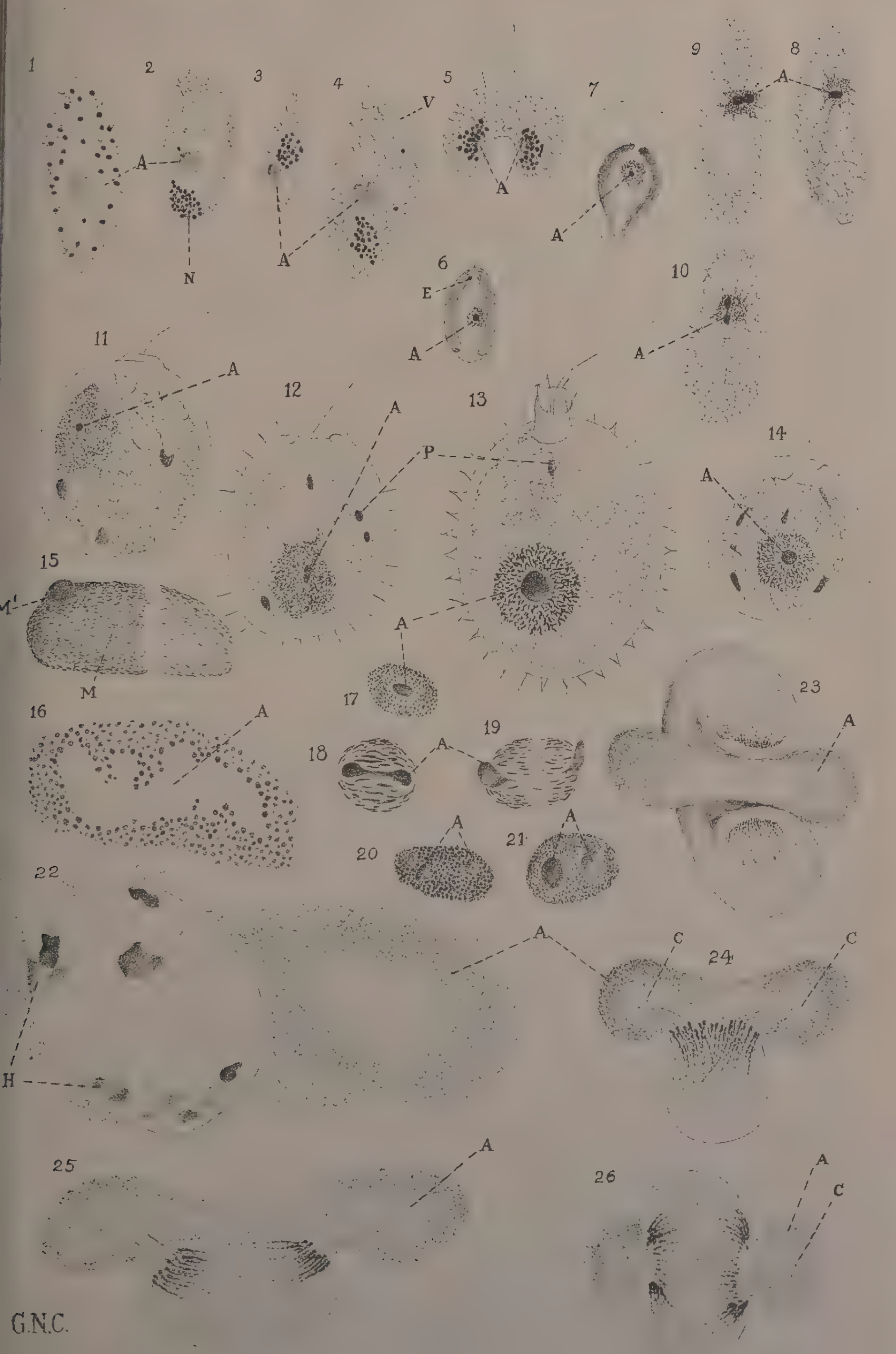
PLATE XXXV.

PLATE XXXV.

PROTOZOAN NUCLEI.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[Annals N. Y. Acad. Sci., XI, No. 17, pp. 401 to 403, October 13, 1898.]

A SIMPLE AND CONVENIENT PHOSPHOROSCOPE.

Wallace Goold Levison.

(Read April 4, 1898.)

In Wright's *Light*[1] there is a description of a phosphoroscope designed for lecture illustration which is attributed to Professor John Tyndall. It consists of a cylinder set in revolution by a crank mechanism before a slit in a light-tight box, through which the light from an electric arc lamp enclosed in the box falls upon the cylinder. The cylinder being coated with coarsely pulverized uranium glass, the audience, in a dark room observes a band of green light across the cylinder the intensity of which increases in proportion to the rapidity of its revolution. This is due to light absorbed by the uranium glass as it passes the slit, and given forth so deliberately as to be still escaping during the time required for more than a half revolution of the cylinder.

Having occasion to use some such simple contrivance in a recent investigation upon this property[2] of minerals, I constructed a modified form of this instrument consisting of a hollow pasteboard cylinder, set in revolution by an electromotor, whereby much greater speed is attained than by a mechanical device. Instead of coating the cylinder directly with the mineral to be examined I dust it in coarse or fine powder upon the surface of sheets of paper brushed over with hot gelatine. These fold around the cylinder and fasten with rubber bands, and are, therefore, interchangeable at pleasure. In other cases I simply fix a single piece of a mineral, either transparent or opaque, upon the surface of the cylinder. At the great speed

[1] Wright (L.), *Light*, London, 1882.

[2] For which the term photofluorescence, in view of the recent experiments of Wiedemann and Schmidt seems to me best adapted.

attained by the electromotor, bands of light are thus obtained from certain minerals which afford perhaps a shorter afterglow than uranium glass. In one or the other of these ways I have obtained a band of green light from willemite, from Franklin, N. J., and a band of crimson light from corundum, from near Franklin, Macon Co., N. C. I have no doubt that other minerals affording too short an afterglow to be at all pronounced with the cylinder revolved by a hand power motor, would be effective with my light cylinder set in rotation at the high speed of an electromotor.

By further modification the apparatus may be used in two other ways. The hollow pasteboard cylinder employed is closed by a solid wooden block at the end which is fixed upon the axle of the electric motor. The other end may be closed with a paper cover, or left open; in the former case I attach to the inside of this cover[1] a spring forceps, by means of which an object such as a diamond, a ruby, or a piece of willemite may be held exactly in the center of the cylinder. The cylinder is provided with a side opening through which the light from a lantern condenser may be focused upon the object in the center of the cylinder when the opening is on the side away from the observer, and through which the side of the object just previously illuminated, may be seen by the observer wholly screened from any light whatever, when the opening is on the side of the cylinder toward the observer. If the object be thus examined in a totally dark room and affords no afterglow, nothing whatever is seen; but if it affords an afterglow, it becomes visible owing to the persistence of vision, with a characteristic colored light when the cylinder rotates with sufficient speed, and its brilliancy increases as the speed of rotation of the cylinder further increases.

In the latter case a similar spring forceps supported upon a suitable stand is introduced through the open end of the cylinder to hold the object, which, therefore, does not partake of the motion of the cylinder. The first form is adapted to both trans-

[1] Modification adopted since the paper was read. Exhibited at the Annual Reception of the Academy [Physics, No. 7], April 13, 14, 1897.

parent and opaque objects, the latter more particularly to transparent objects which, being at rest, are more distinctly seen ; an advantage in the case of cut gems.

In one or the other of these ways I have obtained beautiful results from uranium glass, cut rubies, semi-transparent corundum and willemite. I have not yet had an opportunity to try a diamond affording an afterglow.

It is evident that both opaque and transparent substances may be examined by this instrument, either fixed upon the outside of the cylinder, or held within it, as described ; and in either case its indications are quite sensitive, inasmuch as it may be given so high a speed that only a very small fraction of a second elapses after the object is illuminated and before its presentation in absolute darkness to the eye of the observer. Moreover, in either case, one object may be substituted for another quickly and easily, and the brilliancy obtained from some minerals, especially rubies, is quite surprising.

[ANNALS N. Y. ACAD. SCI., XI, No. 18, pp. 405 to 406, October 13, 1898.]

PHOTOGRAPHED OCULAR MICROMETERS.

WALLACE GOOLD LEVISON.

(Read April 4, 1898.)

OWING probably to the difficulty of starting and stopping ruling machines at cross lines without overrunning them, it has been found difficult to obtain eye piece micrometers ruled in squares, particularly of the design now so much used in water supply investigations for counting and measuring micro-organisms.

It occurred to me that these micrometers might be made easily by photography, and as a test experiment I made some of them by the ordinary simple dry-plate method with some precautions to ensure clear films. An outline drawing 14 centimeters square was first made with India ink and an ordinary drawing pen upon glass coated with gelatin, and one-half the square in each direction was ruled in five equal parts. The small central square formed by the crossing of these lines was then divided by cross lines into twenty-five equal areas according to the plan given in Prof. Albert R. Leed's report on the Brooklyn water.[1]

This drawing was photographed down by an ordinary one-quarter portrait lens with small diaphragm stop, to about five centimeters square on a Stanley dry plate, care being taken to obtain as nearly as possible a black negative with very clear lines. For each micrometer this is again reduced by the same lens to a square of seven millimeters on lantern slide plate, care being taken to develop the lines black, keep the film transparent and avoid scratches.

[1] A. R. Leeds, Report on the Brooklyn Water, published by the Department of City Works of Brooklyn, N. Y., 1897.

The plates thus obtained are cut in circles a little larger than the recess in the eye piece diaphragm in which they are to be used. A cover class is then applied with balsam and xylol and baked for several days until the cement is hard and dry. The circle is finally accurately centered on a lathe and ground to a true circle of the exact size of the recess in the eye piece diaphragm.

In making these photographs the action of halation will cause the lines to be much thinner in the negative than in the original drawing and thicker in the finished positive than in the negative, and moreover it will cause a peculiar thickening where the lines intersect unless the precautions known to expert photographers are employed at each step to counteract this peculiarity of the photographic process.

But even if not wholly obviated, this does not materially interfere with the practical utility of the micrometer. I have no doubt that very accurate and beautiful micrometers may be thus made by the so-called process method which is a wet plate method used for making photo engravings as it affords jet black lines on a particularly clear ground. The lines as I have made them are thicker perhaps than is necessary, but this does not appear to interfere with the use of the micrometer, providing distances are taken from one side of a line to the same side of the next line, and so on throughout the scale.

Eye piece micrometers made by the simple method I have tried appear to be satisfactory for use with any objective, as regards transparency. In fact they seem in some respects to be more satisfactory than ruled micrometers, especially in the circumstance that the lines are black and always distinctly visible and that they can be made with facility of any design desired. For the latter reason they may be valuable not only for measuring micro-organisms but also any class of microscopic objects whatever, as for example the areas of the crystals or grains of minerals in thin sections of rocks and building stones and thereby perhaps estimating their relative proportions.

[ANNALS N. Y. ACAD. SCI., XI, No. 19, pp. 407 to 413, October 13, 1898.]

NOTES ON BERMUDA ECHINODERMS.

HUBERT LYMAN CLARK.

(Read May 9, 1898.)

THE collection of echinoderms made in Bermuda in the summer of 1897 by the New York University party, has been very kindly placed in my hands by Professor Bristol, for examination. Although the collection is in itself a small one, it is of no little interest, as our present knowledge of the echinoderms of Bermuda is very incomplete. So far as I can discover, no attempt has hitherto been made to prepare a complete list of them, so that it has seemed worth while to add to the species in the New York University collection, others which have previously been recorded from the islands, thus making as far as possible a catalogue of the littoral echinoderms of Bermuda. In 1888, Professor Heilprin, of the Philadelphia Academy of Sciences, published in the *Proceedings* of that Academy, a list of the echinoderms, which he and a party of students had collected in Bermuda that summer. The list contains twenty species, six holothurians, six echinoids, six ophiurids and two asteroids. Of the six holothurians, four are described as new to science. The New York University collection contains only eleven species, but of these at least three are additions to Professor Heilprin's list. The principal interest of the collection, however, lies in the light which it throws on Professor Heilprin's "new" species of holothurians, and on one of Professor Verrill's species of starfish.

There are only two species of ASTEROIDS in the collection, but both are of interest. One of them, of which ten specimens lie before me, is the common starfish of the Bermudas. One of its peculiar features is the great variation in the number of arms, one specimen having nine, five having seven and the other four

six, while Professor Heilprin reports having found one or two specimens with only five. The specimens I have agree in every particular with the most careful descriptions of *Asterias tenuispina* Lamk., from the Mediterranean and eastern Atlantic, and, I have no doubt, belong to that species. Verrill has separated the *Asterias* of Bermuda from *A. tenuispina* as *A. atlantica*, on the ground that the proportions of the arms are slightly different and that there are no large single pedicellariæ. Sladen, in his report on the starfishes of the "Challenger" collections, identifies the only *Asterias* from Bermuda as *A. tenuispina* and questions the authenticity of Verrill's species. In the specimens before me the proportions of the arms vary considerably and large single pedicellariæ occur in the ambulacral furrow as in *A. tenuispina*. Accordingly it would appear that *A. atlantica* must be regarded as a synonym of that species. In several of the New York University specimens the prominent spines on the upper surface are rather unusually colored, being strongly tinged with violet. The other starfish, of which there are five specimens in the collection, is *Asterina folium* Ltk., a small pentagonal species found closely adhering to the under side of broken pieces of rock. They are very light colored, almost white, but one is strongly tinged with blue. They agree in all particulars with specimens of the same species from Jamaica.

The two OPHIURIDS are of no especial interest, though one of them has not previously been taken in Bermuda. This is *Ophiura appressa* Say, of which there are three specimens in the collection. They were kindly identified for me and compared with Jamaica specimens by my friend, Mr. Caswell Grave, of the Johns Hopkins University. Of the other species, *Ophionereis reticulata* Ltk., there is a large number of specimens. It seems to be the common brittle-star of the islands.

The four ECHINOIDS are all reasonably common in suitable places, Professor Bristol tells me, and have all been recorded from Bermuda before. They are *Diadema setosum* Gray, *Echinometra subangularis* Leske, *Hipponoë esculenta* Leske and *Toxopneustes variegatus* Lamk. Anyone familiar with the latter urchin as it appears in Jamaica or along our southern coast would never recog-

nize it in these handsome specimens from Bermuda. A close examination, however, shows that the great difference in color is only one of degree. Specimens from Jamaica are green with white markings and with whitish or greenish spines, the latter being often tipped with violet. Now in the Bermuda *Toxopneustes*, violet has become the predominant color, so that all trace of green and white variegation has disappeared. The test has become very dark and the spines are a bright purple violet. This tendency towards violet coloration of spines has already been mentioned in connection with the starfish, *Asterias*, and it is also quite marked in one of the other sea-urchins, *Echinometra*. Specimens of this form from Jamaica are usually reddish-brown of some shade but the spines are often greenish, tipped with violet. Bermuda specimens show this violet coloration of the spines much more plainly. It would be interesting to know what may be the cause of this tendency toward violet among the Bermuda echinoderms; but I have no explanation to offer.

There are only three species of HOLOTHURIANS in the collection but all of these are of considerable interest because of the light which they throw on the "new" species described by Professor Heilprin. Professor Bristol's students report that there are two large species of *Stichopus* common at the Bermudas, and that they are readily distinguishable from each other. This statement agrees with Professor Heilprin's, who has described and figured each of them as a new species. One of them is black and was called *S. diaboli*, but I am sorry to say that of this species there is not a specimen in the collection before me. The other one is less common, is markedly different in color, and was given the name *S. xanthomela* Heilprin. Of this species, I have two specimens in hand, one of which agrees perfectly in color with Professor Heilprin's description, while the other is much darker. It needed but a glance to see that they are the common West Indian form of *Stichopus*, though what that form is to be called it is not easy to decide. A more careful examination of the Bermuda specimens has shown that they agree in all particulars with specimens from Jamaica. After a careful examination of hundreds of specimens of *Stichopus* from Jamaica, both living

and alcoholic, I am convinced that specific differences cannot be distinguished in this genus with any accuracy except in living specimens, and furthermore that coloration is so variable that it is almost useless as a standard in classification. Four species of *Stichopus* have been described from the West Indian area, all of them from alcoholic material, by men who have never visited the West Indies, and they are separated from each other by characters which are seen in a large series of specimens to intergrade in inextricable confusion. For the present however, the commonest West Indian species may bear the name *S. möbii*, bestowed by Semper, and Heilprin's *S. xanthomela* is doubtless the same. According to the latter the Bermuda form has eighteen tentacles, but both of the specimens before me have twenty, while one Jamaica specimen has nineteen and another twenty-one. The normal number of tentacles in *Stichopus* is however twenty, and any other number is merely an individual peculiarity.

The second species of holothurian from Bermuda in my hands is a small one, occurring under broken slabs of rock, and of this there are six specimens. I have compared them with more than a dozen species of small holothurians collected in Jamaica in similar situations, but they do not agree with any of them satisfactorily. After some hesitation, I have decided to refer them provisionally to Ludwig's *Holothuria surinamensis*, as they approach nearest to that species, though the differences are pretty clearly marked. I think it probable that a larger series of specimens will show the Bermuda form to be a new species. Professor Heilprin collected five specimens of a small holothurian, which he refers to *H. floridana* Pourt., but neither in his description nor his plate does he refer to the small rosette-like calcareous bodies, so characteristic of that species and its allies. If they are not present in his specimens, I should think it at least possible that these are the same species as the ones before me. The last of the three species in the New York University collection is obviously either a *Thyrne* or a representative of that section of *Cucumaria* to which Lampert gave the name *Semperia*. There are two specimens about 6 cm. long and agreeing in all particulars with each other. After a careful

examination I refer them without hesitation to Ludwig's *Cucumaria punctata*, described from a specimen collected in Barbadoes. In a few details they differ from that species: the color being apparently different, the stone canal free, only one polian vessel, and the anus armed with five small calcareous teeth. The calcareous buttons are so numerous in some places that the skin is very hard, the layer of buttons being .4 mm. thick. Professor Heilprin describes from a single specimen a new species of *Cucumaria* which he calls *Semperia bermudiensis.* While I have no way of proving that this is the same species as the specimens before me, the differences which he points out between it and Ludwig's *C. punctata* do not seem to me important, and I strongly suspect that *S. bermudiensis* Heilp. ought to be put down as a synonym of *C. punctata* Ludw. I am at a loss to understand what Professor Heilprin means by the "long back processes" of the calcareous ring "for the attachment of the powerful retractor muscles." So far as I know the retractor muscles of *Cucumaria* and *Thyrne* are never attached to the *posterior* prolongations of the radial pieces of the calcareous ring but always to *anterior* prolongations. The latter are quite long in *Cucumaria punctata.*

In the light of these facts, I append the following revised list of the littoral echinoderms of Bermuda, as complete as I have been able to make it. It does not pretend to include the deeper water species collected in the vicinity of the islands by the "Challenger."

CATALOGUE OF THE LITTORAL ECHINODERMS OF BERMUDA.

ASTEROIDS.

1. **Asterias tenuispina** Lamk. = *A. atlantica* Verrill. Common. Collected by all parties.

2. **Asterina folium** Ltk. Not very common. One specimen collected by the "Challenger" and five by the New York University party.

3. **Linckia guildingii** GRAY. Apparently not common. Recorded by Sladen in the "Challenger" report and by Professor Heilprin.

OPHIURIDS.

4. **Ophiactis mülleri** LTK. Two specimens collected by the Philadelphia party.

5. **Ophiocoma crassispina** SAY. One specimen taken by the Philadelphia party.

6. **Ophiocoma pumila** LTK. Collected by the "Challenger" and by the Philadelphia party.

7. **Ophiomyxa flaccida** LTK. One specimen taken by the Philadelphia party.

8. **Ophionereis reticulata** LTK. Abundant. Recorded by all parties.

9. **Ophiostigma isacantha** SAY. Two specimens taken by the Philadelphia party,

10. **Ophiura appressa** SAY. Three specimens taken by the New York University party.

ECHINOIDS.

11. **Cidaris tribuloides** BL. Reported common by the Philadelphia party.

12. **Diadema setosum** GRAY. Common. Collected by all parties.

13. **Hipponoë esculenta** LESKE. Not uncommon. Collected by all.

14. **Echinometra subangularis** LESKE. Common. Collected by all.

15. **Toxopneustes variegatus** LAMK. Common. Collected by all.

16. **Mellita sexforis** AG. Said to be common, but not actually collected by either the Philadelphia or New York parties. Recorded from Bermuda by Agassiz.

17. **Echinoneus semilunaris** LAMK. Reported from Bermuda by Agassiz in his "Revision of the Echini" and in the "Challenger" report.

18. **Brissus unicolor** Kl. Reported from Bermuda by Agassiz.

HOLOTHURIANS.

19. **Cucumaria punctata** Ludw. Two specimens collected by the New York University party.

20. **Cucumaria (Semperia) bermudiensis** Heilp. A very doubtful species described from a single specimen taken by the Philadelphia party.

21. **Holothuria floridana** Pourt. Five specimens collected by the Philadelphia party.

22. **Holothuria captiva** Ludw. Two specimens collected by the Philadelphia party.

23. **Holothuria abbreviata** Heilp. A very doubtful species described from a single specimen, probably an abnormal individual of the preceding species, collected by the Philadelphia party.

24. **Holothuria surinamensis** (?) Ludw. Six specimens, collected by the New York University party, are referred to this species with much hesitation.

25. **Stichopus diaboli** Heilp. Reported as very common.

26. **Stichopus möbii** Semper. = **S. xanthomela** Heilp. Reported as quite common.

27. **Stichopus haytiensis** Semper. Reported from Bermuda by Dr. Theél from a single specimen collected by the "Challenger." I am inclined to think it may be the same species as the preceding.

28. **Synapta vivipara** Oerst. Recorded from Bermuda by Dr. Theél in the "Challenger" report under the name *S. picta.* Dr. Theél also has numerous other specimens from the Bermudas.

Of the above twenty-eight species, four or five of the holothurians are in doubt, so that the need of larger and more complete collections is very obvious. Of the remaining twenty-two or three species, all but one or two are distinctly West Indian, so that it is only fair to expect the discovery of many more, by more careful and systematic collecting.

[ANNALS N. Y. ACAD. SCI., XI, No. 20, pp. 415 to 430, October 13, 1898.]

ADDITIONS TO THE PALÆOBOTANY OF THE CRETACEOUS FORMATION ON STATEN ISLAND. NO. II.

ARTHUR HOLLICK.

(Read May 16, 1898.)

[PLATES XXXVI–XXXVIII.]

IN two papers upon this subject previously published our knowledge of the Cretaceous flora of Staten Island was brought up to the year 1892.[1] Since that time considerable additional material has been collected, including several species not before recorded from the island, which have been the subject of notes and memoranda read before the Natural Science Association of Staten Island and published from time to time in its Proceedings.

The object of the present paper is to describe this material as a whole and also to indicate certain modifications of views previously expressed, due to information acquired since the other contributions to the subject were issued.

All the specimens were found in hardened ferruginous clay concretions or shaly fragments, in connection with the terminal moraine. None of the specimens was found in place, although they must have been derived from Cretaceous strata in the immediate vicinity, either on the island or on the adjacent mainland. Some of those from Tottenville and Prince's Bay may have been from the latter source, but the Arrochar specimens were undoubtedly native to the island, although disturbed from their original position.

It was previously taken for granted that all the cretaceous

[1] The Palæontology of the Cretaceous Formation on Staten Island. *Trans. N. Y. Acad. Sci.*, XI (1892), 96–104; Pl. I–IV.

Additions to the Palæobotany of the Cretaceous Formation on Staten Island. *Ibid.*, XII (1892), 28–39; Pl. I–IV.

strata on Staten Island were continuations of those at Perth Amboy and Woodbridge, and that the fossil plants found in them or derived from them would prove to be identical with those of the mainland. Such, however, has not been found to be the case, and this fact has seemed to indicate that some of the strata from which the Staten Island plants were derived may represent a different and presumably a higher member of the Amboy clay series than do those represented at the New Jersey localities mentioned.

Many of the species are identical, but a number of those found on Staten Island have not yet been discovered in the New Jersey clays, although these have been quite extensively exploited and hundreds of specimens have been collected from them; and further, some of the species most common in New Jersey are conspicuous by their absence or rarity on Staten Island.

As is well known the Cretaceous clays of New Jersey extend across the State with a general northeast and southwest strike and a dip towards the southeast of about fifty feet to the mile. Those which outcrop furthest to the northwest are therefore the lowest or oldest of the series. If a geological map of New Jersey be examined and the trend of the clay outcrops be theoretically extended on to Staten Island, it may be readily seen that the lower beds, represented by those at Woodbridge, Sayreville, Perth Amboy and possibly South Amboy, would strike the western shore of Staten Island in the vicinity of Tottenville and Kreischerville, while the upper beds, represented by those in the vicinity of Cheesequakes creek, would strike along the southern shore of the island from Tottenville to Arrochar.

This probability is further strengthened by the fact that marl bed fossils have been found in the moraine at the latter locality, showing that strata even higher than the upper members of the clay series are or once were represented there.

From a consideration of these facts and other similar ones in connection with the Cretaceous clays on Staten Island, Long Island, Block Island and Martha's Vineyard, the name "Island

Series" was given by Dr. Lester F. Ward to the strata represented on these islands.[1]

The "Island Series" would therefore lie above the Amboy clays as described by Newberry,[2] and below those of the clay marls at Clifford, as described by me in a recent paper.[3]

The sequence of the strata and their relations to the localities where they are prominently exposed may be understood from the following table:

Geological Horizons.	Strata.	New Jersey Localities.
Matawan.		Cliffwood.
Upper Potomac (Amboy Clays).	Island Series.	Morgans. (?)
	Albirupean Series.	South Amboy. Perth Amboy. Sayreville. Woodbridge.
?	("Iron Ore Series"?)	
Middle Potomac.	Acquia Creek Series.	Not known in New Jersey.
Basal Potomac.	Mount Vernon Series. Rappahannock Series. James River Series.	

Whether or not all of Dr. Ward's conclusions will stand, appears to me, will depend upon future investigation. Thus far I have failed to find the equivalent of the Island Series on the mainland of New Jersey in the region where it should theoretically occur, nor have the ferruginous concretions and fragments, by which the series is characterized on the islands, been found there, and the fact of their absence on the mainland, and their presence on the islands only in connection with the terminal

[1] The Potomac Formation, 15th *Ann. Rept. U. S. Geol. Surv.*, 335, 336.

[2] The Flora of the Amboy Clays, *Monog. U. S. Geol. Surv.*, XXVI.

[3] The Cretaceous Clay Marl Exposure at Cliffwood, N. J., *Trans. N. Y. Acad. Sci.*, XVI (1897), 124–136.

moraine, has led me to think that they are not characteristic of the series except as representing fragments of clay strata which were originally in a plastic condition but which have become hardened by oxidation after having been torn up and made part of the morainal material. This view is further strengthened by the fact that these concretions and fragments may be found in the moraine in every stage of development from masses of soft clay with only a thin shell of limonite on the outside to those which are hardened throughout. Many of the hardest fragments also exhibit beautifully defined planes of shearing or slipping, evidently accomplished before the process of hardening had been completed. In several localities, notably at Glen Cove, Long Island, and at Gay Head, Martha's Vineyard, the shaly fragments and concretions occur in the Cretaceous clay strata, but these strata are greatly contorted and have been subjected to similar conditions to those which have wrought the changes noted in the mixed morainal material. The disturbance of the strata would naturally expose them to the same oxidizing influences and would cause portions of them to be converted into hardened seams or assist in the formation of concretions. So that until we find the strata upon the mainland with such hardened seams, fragments or concretions in place and containing representatives of the same flora, the most reasonable explanation of their occurrence throughout the morainal region of the islands would seem to be that it is due to oxidation caused by the disturbance wrought there by glacial action.

Cretaceous Plants of Staten Island.

In the following list Nos. 4, 5, 8, 9, 10, 11, 12, 14, 15 and 16 have not before been reported from Staten Island, and No. 12 represents a species here described for the first time.

1. **Moriconia cyclotoxon** Deb. & Ett.

(Plate XXXVII, Fig. 8.)

Moriconia cyclotoxon Deb. & Ett., Urwelt. Acrob. Kreidegeb. Aachen und Maestricht, p. 59 (239), Pl. VII, Figs. 23–27.

Locality: Prince's Bay, Staten Island.

2. **Thinnfeldia Lesquereuxiana** Heer.

(Plate XXXVI, Fig. 6.)

Thinnfeldia Lesquereuxiana Heer, Fl. Foss. Arct., Vol. VI, Abth. II, p. 37, Pl. XLIV, Figs. 9, 10; Pl. XLVI, Figs. 1–11, 12a and b.

Locality: Tottenville, Staten Island.

3. **Populus Harkeriana** Lesq. (?)

(Plate XXXVI, Fig. 8.)

Populus Harkeriana Lesq., Fl. Dak. Gr., p. 44, Pl. XLVI, Fig. 4.

Although somewhat imperfect in outline, this specimen appears to agree in all essential particulars with this species and seems to warrant at least a provisional reference to it.

Locality: Tottenville, Staten Island.

4. **Salix inæqualis** Newb.

(Plate XXXVIII, Fig. 4a.)

Salix inæqualis Newb., Fl. Amboy Clays, p. 67, Pl. XVI, Figs. 1, 4, 6; Pl. XVII, Figs. 2–7.

Locality: Arrochar, Staten Island.

5. **Myrica longa** Heer.

(Plate XXXVIII, Fig. 6.)

Proteoides longus Heer, Fl. Foss. Arct. Vol. III (Kreidefl.), p. 110, Pl. XXIX, Fig. 8b; Pl. XXXI, Figs. 4, 5.

Myrica longa Heer, *ibid.*, Vol. VI, Abth. II, p. 65, Pl. XVIII, Fig. 9b; Pl. XXIX, Figs. 15–17; Pl. XXXIII, Fig. 10; Pl. XLI, Fig. 4d.

Locality: Arrochar, Staten Island.

6. **Ficus Woolsoni** Newb. (?)

(Plate XXXVII, Fig. 9.)

Ficus Woolsoni Newb., Fl. Amboy Clays, p. 70, Pl. XX, Fig. 3; Pl. XXIII, Figs. 1–6.

It is with considerable hesitation that I have provisionally referred this fragmentary specimen to this species. Fig. 6 of Plate XXIII, seems, however, to approach it quite closely. A similar specimen was also found in the clay at Kreischerville, which I referred provisionally to the same species (Trans. N. Y. Acad. Sci., Vol. XII (1892), p. 33, Pl. II, Fig. 1) and there can be hardly any doubt that our two specimens represent one and the same species.

Locality: Tottenville, Staten Island.

7. **Protæoides daphnogenoides** Heer.

(Plate XXXVI, Figs. 1–3.)

Protæoides daphnogenoides Heer, Phyl. Crét. Nebraska, p. 17 Pl. IV, Figs. 9, 10.

This species was identified in the Amboy clays of New Jersey by Newberry and whether his specimens are correctly referred or not, there can be no doubt of the identity of our specimens with those from New Jersey. (See Fl. Amboy Clays, Pl. XXXII, Figs. 11, 13, 14.)

Locality: Tottenville, Staten Island.

8. **Myrsine elongata** Newb.

(Plate XXXVIII, Figs. 3, 4b and c.)

Myrsine elongata Newb., Fl. Amboy Clays, p. 122, Pl. XXII, Figs. 1–3.

Locality: Arrochar, Staten Island.

9. **Andromeda Parlatorii** Heer.

(Plate XXXVII, Fig. 7.)

Andromeda Parlatorii Heer, Phyl. Crét. Nebraska, p. 18, Pl. I, Fig. 5.

For purposes of comparison the specimens figured by Newberry (Fl. Amboy Clays, Pl. XXXI, Figs. 1–7; Pl. XXXIII, Figs. 1, 2, 4, 5) are better than the type specimen figured by

Heer. This is particularly the case in regard to Figs. 2 and 4, Pl. XXXI, above quoted.

Locality: Tottenville, Staten Island.

10. **Hedera** sp.?

(Plate XXXVIII, Fig. 5.)

This specimen is too fragmentary for more than a generic reference. It may possibly be a small specimen of *H. primordialis* Sap., as depicted by Newberry in the Flora of the Amboy Clays, Pl. XXXVII, Figs. 1–7.

Locality: Tottenville, Staten Island.

11. **Aralia rotundiloba** Newb. (?)

(Plate XXXVIII, Fig. 2.)

Aralia rotundiloba Newb., Fl. Amboy Clays, p. 118, Pl. XXVIII, Fig. 5; Pl. XXXVI, Fig. 9.

The obliteration of the lobing in this specimen renders accurate determination impossible. It may, perhaps, also be compared with *Cissites ingens* Lesq. (Fl. Dak. Gr., Pl. XIX, Figs. 2, 2a), or with *C. formosus* Heer, as identified by Newberry. (Fl. Amboy Clays, Pl. XLVII, Figs. 1–8.)

Locality: Tottenville, Staten Island.

12. **Pistacia Aquehongensis** n. sp.

(Plate XXXVI, Fig. 5.)

Leaf entire, linear-elliptical in outline, about ¾ in. long by ¼ in. wide; nervation finely and uniformly pinnate, secondaries leaving the midrib at a somewhat obtuse angle, closely parallel and connected near the margin by cross nervation in a series of angles.

The specific name refers to "Aquehonga," the Indian name for Staten Island.

This leaf is closely similar to *P. aquensis* Sap. (Ann. Sci. Nat., Ser. V. Bot., Vol. XVIII (1873), p. 105, Pl. XV, Figs. 1–24), which, however, is a Tertiary species.

Locality: Tottenville, Staten Island.

13. **Sapindus Morrisoni** Lesq.

(Plate XXXVI, Fig. 4.)

Sapindus Morrisoni Lesq. Cret. & Tert. Fl., p. 83, Pl. XVI, Figs. 1, 2.

Locality: Prince's Bay, Staten Island.

14. **Sterculia Snowii** Lesq. (?)

(Plate XXXVII, Fig. 4.)

Sterculia Snowii Lesq. Fl. Dak. Gr., p. 183, Pl. XXX, Fig. 5; Pl. XXXI, Figs. 2, 3; Pl. XXXII; Pl. XXXIII, Figs. 1–4.

This specimen is too fragmentary for more than provisional reference.

Locality: Tottenville, Staten Island.

15. **Sterculia** sp.?

(Plate XXXVII, Fig. 5.)

Locality: Tottenville, Staten Island.

16. **Pterospermites modestus** Lesq.

(Plate XXXVII, Fig. 6.)

Pterospermites modestus Lesq. Fl. Dak. Gr., p. 186, Pl. LVIII, Fig. 5.

Locality: Tottenville, Staten Island.

17. **Magnolia longifolia** Newb. (?)

(Plate XXXVII, Fig. 3.)

Magnolia longifolia Newb. Fl. Amboy Clays, p. 76, Pl. LV, Figs. 3, 5; Pl. LVI, Figs. 1–4.

This specimen is evidently a fragment of a large leaf, with the nervation of *Magnolia*, and its provisional reference to one of the Amboy clay species seems to be justifiable.

Locality: Tottenville, Staten Island.

18. **Dewalquea Groenlandica** Heer. (?)

(Plate XXXVI, Fig. 7.)

Dewalquea Groenlandica Heer. Fl. Foss. Arct., Vol. VI, Abth. II, p. 87, Pl. XXIX, Figs. 18, 19; Pl. XLII, Figs. 5, 6; Pl. XLIV, Fig. 11; *ibid.*, Vol. VII, p. 37, Pl. LXII, Figs. 5, 6.

The reference of our specimen to this species is questionable. Amongst all of Heer's figures the only one with which it can be satisfactorily compared is Fig. 6, Pl. LXII, above quoted. Nevertheless, as our specimen is apparently identical with those provisionally referred to the species in the Flora of the Amboy Clays (p. 129, Pl. XLI, Figs. 2, 3, 12), I have thought it best to retain the name.

Locality: Tottenville, Staten Island.

19. **Tricalycites papyraceus** Newb.

(Pl. XXXVII, Figs. 1, 2.)

Tricalycites papyraceus Newb., Fl. Amboy Clays, p. 132, Pl. XLVI, Figs. 30–38.

Locality: Tottenville, Staten Island.

20. **Rhizomorphs.**

(Pl. XXXVIII, Fig. 1.)

I use the term Rhizomorph in the same sense as it was originally used by the late Dr. J. I. Northrop, in describing similar cylindrical structures in the coral rocks on the island of Nassau. (Notes on the Geology of the Bahamas, *Trans. N. Y. Acad. Sci.*, Vol. X (1890), p. 16.) It has no connection with the fungus genus *Rhizomorpha*.

Amongst the commonest of the fossil remains found in the hardened clay nodules in the drift at Tottenville are those which I have included under the comprehensive name of Rhizomorphs. They usually consist of limonite tubes, concretionary in structure, sometimes hollow, sometimes containing lignite or pyrite. Occasionally the lignite has no casing of limonite around it.

They invariably extend through the rock at, or nearly at, right angles to the plane of stratification and are either straight or sparingly branched. Where the ends appear on the surfaces of the rock these give rise to little pits, usually encircled by the rims of the limonite tubes. On breaking one of these nodules open the structure and arrangement of the remains may be observed.

I have never seen any fossils in the Cretaceous clays which are comparable to them, but roots of living plants which have found their way down into ferruginous clays and sands often produce very much the appearance of our specimens, and I am inclined to think that these rhizomorphs represent the lignified remains of former living roots, which were retained in their original positions after the clay had been torn up and transported. During the subsequent hardening of the clay and the oxidation of its contents, iron-bearing water followed along the roots, gradually depositing a tube of limonite, while the vegetable tissue was either destroyed or converted into lignite. From this point of view our rhizomorphs would represent post-Cretaceous preglacial vegetation.

PLATE XXXVI.

PLATE XXXVI.

CRETACEOUS PLANTS FROM STATEN ISLAND.

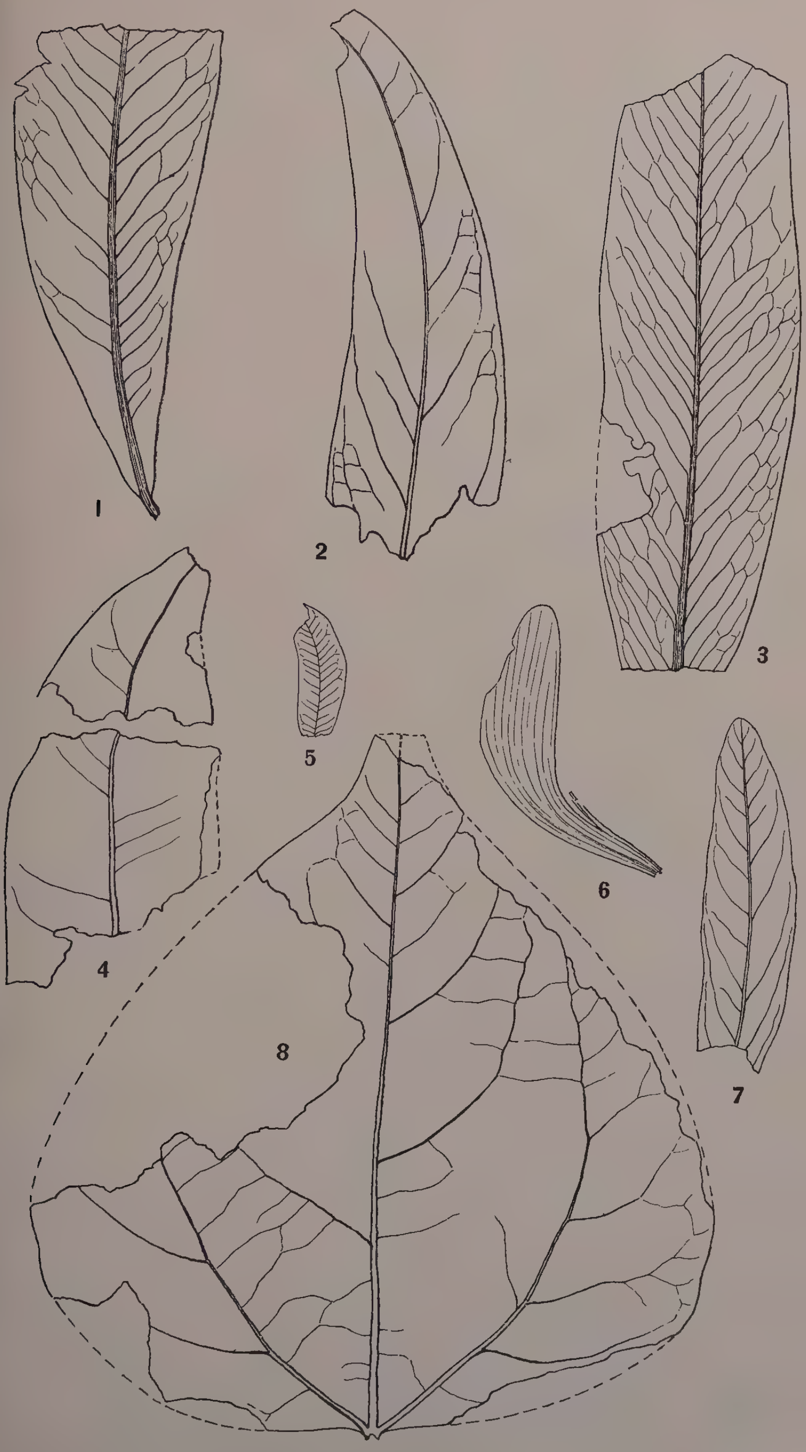
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PLATE XXXVII.

PLATE XXXVII.

CRETACEOUS PLANTS FROM STATEN ISLAND.

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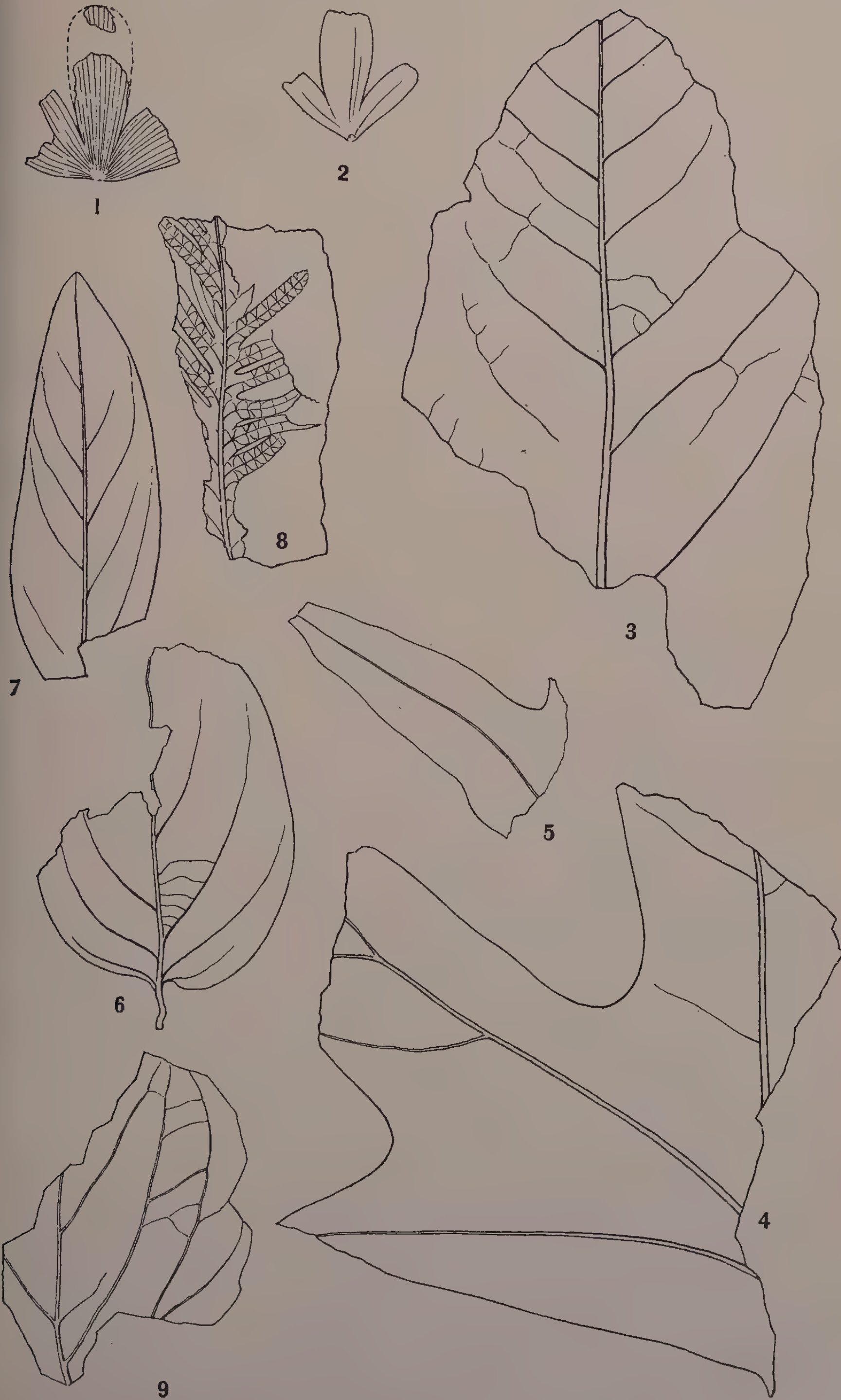
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PLATE XXXVIII.

PLATE XXXVIII.

CRETACEOUS PLANTS FROM STATEN ISLAND.

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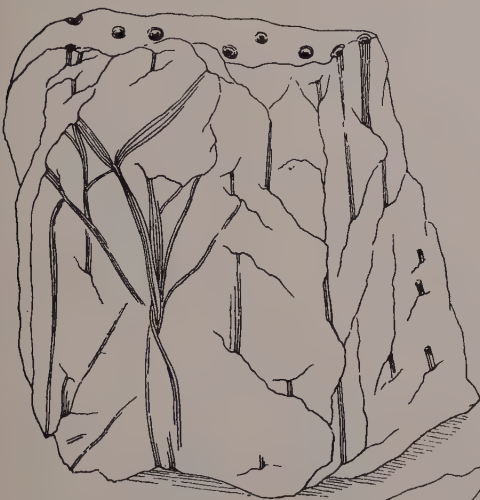

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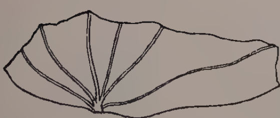

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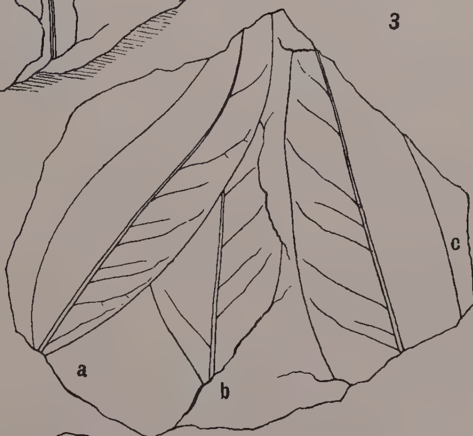

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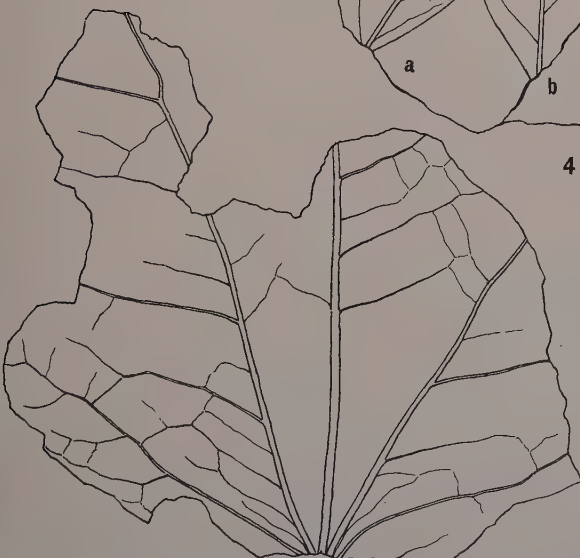

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[ANNALS N. Y. ACAD. SCI., XI, No. 21, pp. 431 to 441, December 17, 1898.]

THE LATTER PART OF LUCRETIUS, AND EPICURUS περὶ μετεώρων.

E. G. SIHLER.

(Read March 28, 1898.)

THE charm of Lucretius is perennial. The source of it, however, is rather complex. That his work is the foremost didactic poem of antiquity is admitted. That his manipulation of the possibilities of the Latin tongue stamps it, as Teuffel says, as the production "Eines Sprachgewaltigen" few would gainsay. That his *exordia* and many of his digressions really are meant by the *lumina* of Cicero's judgment in his letter to his brother Quintus (2, 11) seems most probable. And still stronger than these is the tremendous earnestness of the man. We have a distinguished Epicurean in the generation after L., Horace of Venusia. To him, too, we may trace that blending of morals, of quasi-religious conviction and strictly philosophical tenets, which constituted adherence to the one or the other of the two most prominent sects of the day: the Epicurean and the Stoic. These conditions Horace evidences most frankly in his earlier writings, *e. g.*, in the Iter Brundusinum, I, Sat. 5, 97.

dein Gnatia lymphis
Iratis extructa dedit risusque iocosque,
Dum flamma sine tura liquiscere limine sacro
Persuadere cupit. Credat Judaeus Apella
Non ego: namque deos didici securum agere ævum,
Nec, siquid miri faciat natura, deos id
Tristes ex alto caeli demittere tecto.

The vanity of concern for the utter extinction implied in the mortality of the soul is iterated in his Odes, as is the vanity of all human passions; the good-natured banter of criticism of Stoic exaggeration comes naturally from an Epicurean; but the

fearless and uncompromising attack on the Etruscan religion of his country is not sounded by the pensioner of Maecenas, and the poet who composed for the princeps the Carmen Saeculare and supported distinctly the social and religious reforms so dear to Augustus (recorded too as the latter's dearest aspirations in the Monumentum Ancyranum) could not well make a propaganda—for Epicureanism. It is different with Lucretius. His tremendous earnestness is coupled with a humility and reverence for the person and doctrine of Epicurus which I need not substantiate here in detail, I, 66–79, III, 1 sqq, and particularly III, 1042

> ipse Epicurus obit decurso lumine vitæ
> qui genus humanum ingenio superavit et omnis
> restincsit, stellas exortus ut ætherius sol . . .

and the much quoted lines V, 8, sq.

> deus ille fuit, deus, inclyte Memmi
> qui princeps vitæ rationem invenit eam quæ
> nunc appellatur sapientia . . .

As to the Greek sources of Lucretius: was there anything beside Epicurus himself? If so, what? If not, which writings of E.? Then too: did he base it all on the 37 bb. of E. *περὶ φύσεως*? The exhaustive grouping of every shred of Epicurean doctrine by H. Usener, of Bonn, in his Epicurea, Leipzig, 1887, with the critical edition of the text of book X, of Diogenes Laertius is a monument of erudition . . . still, inasmuch as Epicurus' doctrine is stated there with very great conciseness as a summary digest for the conning of confirmed disciples and not with explicit clearness nor argumentative breadth, the temptation has always been great for students of the subject to make Epicurus' letter to Herodotus a "source" of L.

The exact mode in which Lucretius used the main work the 37 bb. *περὶ φύσεως* will probably never be known, inasmuch as, although there were three complete copies of Ep. *περὶ φύσεως* in the villa of Piso at Herculaneum, the deciphered fragments from the carbonized rolls are entirely too scanty to permit inferences; if Philodemus, a second-rate writer, was represented

with his endless volumina in that Epicurean library, are we to believe that a Lucretius was content with a perusal of anything short of the great dogmatic work of the master? It is not likely. This, too, is made more probable by the substantial elements of controversial analysis or censure directed against other schools, and particularly against the Stoics, although the latter are never mentioned by name throughout the work of Lucretius. These controversial elements certainly were not in themselves attractive to such Roman readers as were to be made proselytes of the sect. Therefore, I do not see how the parallels between the letter to Herodotus and between Lucretius, elaborated by *Ivo Bruns* in his Lucrezstudien, Freiburg, Tuebingen, 1884, prove anything in this respect. Nor am I convinced of the main thesis of Bruns, that Lucretius, in the course of the elaboration of his work, determined quietly to omit or remove the treatment of the theory of cognition, *τὸ κανονικόν*, of the system of Epicurus.

Why should we assume that the treatment of the *Κανών* was an essential part of the 37 bb *περὶ φύσεως* when, as we see in Diogenes L. X., 27, there was a distinct volume *περὶ κριτηρίου ἢ Κανών*....

Lucretius essayed to show that this physical theory truly emancipated the souls of men from fear of death and from all the terrors with which the traditional mythology had invested the Inferno, that it secured that peace of soul which in the Epicurean conception is essentially negative, freedom from all the passions whether involved in the persuit of wealth, of sexual indulgence [hence the appendix of book IV in L.,] or of political preëminence. This is "purgare pectora" L. VI, 24. Now books I–IV substantially present in sequence what Epicurus called *ἡ γνήσιος φυσιολογία*: [letter to Pythocles, Diog. L. 10, 85]. But books V and VI of L. are apt to make at first the impression of a mass of unrelated or ill-related matter. As for book V the very exordium 55 sqq. states distinctly a complex theme: the creation of organic beings, persistence of created types, essentially physical nature of mind: deception of man by visions. My next theme [rationis ordo]: this organic universe is perish-

able [Diog. L. 10, 74 φθαρτοὶ οἱ κόσμοι]. Earth, sky, sea, stars, sun and moon established by that association of matter, creation of living beings out of the earth [primitive civilization], origin of speech, religion, absence of conscious purpose in the movement of heavenly bodies, no teleology: celestial mechanics directed by no divine providence.

These themes are actually found in the book, although in a somewhat different order. Beginning with V. 509 sqq. we begin to notice that characteristic mark of Epicurus' treatment of "τὰ μετέωρα," viz., the advancement of two or more theories to explain phenomena so radically different from the absolute positivism of the atomistic physical system proper, of boks I.–IV. But it may be more instructive to present at first tables showing the themes and the sequence of themes in Epicurus and in Lucretius.

EPICURUS TO PYTHOCLES.

Diog. Laert. X. 85 sqq.
Sun, moon and "other stars"
Size of sun.
Decline and filling of the moon.
Face in moon.
Eclipses.
τάξις περιόδου.
Length of night and day.
Clouds.
Rain.
Thunder.
Lightning.
Thunderbolt.
Waterspouts.
Earthquakes.
Winds.
Hail.
Snow.
Dew.
Hoarfrost.
Ice.

LUCRETIUS V.

509. Motion of stars.
564. Size of Sun.
Heat of Sun.
619. Sun's mutation of course in the year.
650. Night,
656. Periodicity of sunrise.
680. Correlation of day and night in length.
705. moon's phases.
751. Eclipses.
(771–779 resumé.)

BOOK VI.

96. Thunder.
219. Lightning: optical phenomena.
379. Lightning: destructive phenomena.
451. Clouds.
495. Rain.

Rainbow.
Halo of moon.
Comets.
Slower movements of some stars.
Meteors.
Seasons. (?)

527. Snow, hail, hoarfrost, ice. (A few lines only.)
535. Earthquakes.
639. Etna.
713. Nile. (explanation of summer-rise.)
738. Exhalation of Avernus.
840. Puzzling changes of temperature.
906–1082. Magnet.
1090. Epidemics in general.
1138. The plague at Athens. 430–429.
(Paraphrase of Thucyd. II 47–55.)

The most striking thing in the letter to Pythocles is this: The interest of Epicurus in the explanation of these phenomena is not a scientific or even a positive one: it is mainly negative; to furnish *natural* explanations, an assortment of two or three or four or even more, sometimes without much, if any, indication which to prefer as long as the idea of any divine will or agency as a factor was utterly cancelled from the problem; cf., also, in letter to Herodotus § 76 *καὶ μὴν ἐν τοῖς μετεώροις φορὰν καὶ τροπὴν καὶ ἔκλειψιν καὶ ἀνατολὴν καὶ δύσιν καὶ τὰ σύστοιχα τούτοις μήτε λειτουργοῦντός τινος νομίζειν δεῖ γίνεσθαι καὶ διατάττοντος ἢ διατάξαντος καὶ ἅμα τὴν πᾶσαν μακαριότητα ἔχοντος μετὰ ἀφθαρσίας* . . . and so in the letter to Pythocles § 97 of the course of the sun . . . *καὶ ἡ θεία φύσις πρὸς ταῦτα μηδαμὴ προσαγέσθω ἀλλὰ ἀλειτούργητος διατηρείσθω καὶ ἐν τῇ πάσῃ μακαριότητι, ὡς εἰ τοῦτο μὴ πραχθήσεται ἅπασα ἡ περὶ τῶν μετεώρων αἰτιολογία ματαία ἔσται.* And further recurring to the element of supernatural cause 87–, "*ἐπὶ τὸν μῦθον καταρρεῖ.*" And § 115, speaking of other possible modes of explaining meteora: "*καὶ ἄλλοι δὲ τρόποι εἰς τὸ τοῦτο τελέσαι ἀμύθητοί εἰσιν.*"

When we turn away from this general negative bias of this summary *μετεωρολογία* we are met by a curious and puzzling characteristic.

These phenomena, according to Epicurus, according to their very nature, are unattainable to our positive knowledge; many explanations are possible for each of them as a rule, one is as good as the other; their knowledge is a mere inferior corollary to the system of atomism proper, ἡ γνήσιος φυσιολογία (Diog. L. 10. 85). The aim of this αἰτιολογία is not scientific precision, nor the satisfaction of the craving for accurate knowledge; no, here too it is (§85) ἀταραξία; these themes belong to an entirely different category from the (§ 86) τῶν ἄλλων φυσικῶν προβλημάτων κάθαρσις, *e. g.*, that the universe is material and intangible (ἀναφής) as to its fundamental substance (*i. e.*, as to the atoms), and that the atoms are the material principle, principles which are in *absolute* harmony with phenomena; not so, however, with μετέωρα; ,ἀλλὰ ταῦτά γε πλεοναχὴν ἔχει καὶ τῆς γενέσεως αἰτίαν καὶ τῆς οὐσίας ταῖς αἰσθήσεσι σύμφωνον κατηγορίαν.

The main point is not to adopt and persist in any *one* explanation, but give equal authority to them all as long as none of them is in hisharmony with parallel or analogous processes from the spheres of our actual empirical perception and observation (ἐναργήματα § 93)...

In one passage § 94 he refers to the adoption of the single or exclusive as "being smitten" with it—cf. § 98 (καταγαπᾶν) as a folly of him who knows not the (§ 113) limits of human survey. And so—a brief illustration must suffice—*e. g.*, he gives *four* explanations of the changes of sun and moon, and speaks with scorn of the computations of professional astronomers as (§ 93) τὰς ἀνδραποδώδεις ἀστρολόγων τεχνιτείας. . . . of the decline and increase of the moon he offers not less than *six*; explanations, of clouds (§ 99), *four*; of rain, *four*; of thunder, *five* (§ 100); of lightning § 101–102, *seven*; of earthquakes, *three*; with a fourth collective which recurs frequently. It would be mere iteration to go through the whole list.

This easy eclectic attitude towards the real solution of these phenomena, this absolutely unscientific, nay childish, position as over against exact science, naturally brought Epicurus and his school into very glaring contrast as over against the positive attainments of the Peripatetic and Stoic schools.

And so this particular matter well illustrates the attitude of Epicurus and his school to technical culture or towards the cultivation of technical knowledge. Usener has collected the passages: Epicurea, p. 170, sqq. Cf. particularly Diog. 10, 6 *παιδείαν δὲ πᾶσαν, μακάριε, φεῦγε, τἀκάτιον ’αράμενος* and Quintil. 12, 2, 24, "fugere omnem disciplinam." But, we are all told, there are doubts as to the genuineness of the letter to Pythocles, so that Usener, while critically editing it with the other two letters, brackets the title. This is due to a notice of Philodemus in the Herculanean papyri, 2d collation, Tom. I, fol. 152, with Usener's supplements, p. 34.

> "ὑποψ[ία]ν τιν[ὰ] [λα]μβάν[ει]ν ‘ως περὶ τινῶν
> ’επίστολ[ῶν] καὶ τῆς [πρὸς Πυθ]οκλέα π[ε]ρὶ[με]τεώρων
> ’επιτομῆς καὶ τοῦ περὶ ’αρετῶν κτὲ . . .."

The notice of Philodemus, who was a close contemporary of Lucretius and intimate friend of Calpurnius Piso, really is, in the first place, a prima facie proof that this piece of Epicurean writing *existed in his day* and had a place among the works of Epicurus. Further, the summaries must have (like the *κυρίαι δόξαι*) enjoyed a much greater vogue than the bulky works of Epicurus; they were evidently studied and passed on from generation to generation in a school in which the *ipse dixit* of the master was zealously maintained as the standard of true doctrine. It is natural, on the other hand, and most probable that a man of real attainments and wide knowledge like Philodemus had little love for this weakling among the intellectual progeny of the son of Neocles, and would have been glad to have it neglected or cast aside as a bastard.

The genuine and profound indifference of Epicurus towards this entire sphere of themes I need not emphasize again; it is unfair to demand (as Usener does) more apt arrangement and fitness in the succession of themes—or what succession of themes would Usener postulate? The strongest argument for the genuineness of this second-rate product of Epicurus, however, is afforded by the parallel of Lucretius' themes.

He is not (as Epicurus did not) desiring to write an exhaus-

tive or systematic treatise on physical phenomena both normal and abnormal ; at the first reading of the greater part of Book V and all of Book VI one cannot suppress a feeling that system is cast to the winds and to miss that rigid, comparatively speaking, that rigid sequence of treatment which is so unmistakable in the general unfolding of Epicurean doctriue in Books I–IV. Cf. Munro's commentary on Lucretius V, 533. And with Epicurus' incessant railing against the postulate of *onc* explanation (*τὸν μοναχῇ τρόπον*, § 95 l. c. and § 113 *τὸ δὲ μίαν αἰτίαν τούτων ἀποδιδόναι, πλεοναχῶς τῶν φαινομένων ἐκκαλουμένων, μανικόν ἐστι*). Cf. Lucretius V, 620, "*non* inquam, *simplex* his rebus reddita causast."

"729 [of two different astronomical theories]
"proinde quasi id fieri nequeat quod pugnat uterque
"aut minus hoc illo sit cur amplectier ausis."

And 751.

Solis item quoque defectus lunaeque latebras
pluribus e causis fieri tibi *posse* putandumst.

And so again in book VI, 703 sqq., the theory of *αἰτιολογία* is advanced even more clearly :

" Sunt aliquot quoque res quarum *unam* dicere *causam* non satis est, *verum pluris, unde una tamen sit* ; as f. e. when you see the dead body of a man lying at *a distance* [*i. e.*, precluding a close and direct inspection on our part] ; there it behooves us to exhaust the entire range of contingencies through which a man may perish ; although we cannot, at that distance, prove any particular single one : the sword, or frost, or disease, or poison. And so we find the same plurality of explanation in Lucretius : positive and exclusive asseverations in this sphere are impossible.

V 526 nam quid in hoc mundo sit eorum *ponere certum difficile est ;* sed quid possit fiatque per omne [das All].

in variis mundis variā ratione creatis
id doceo *pluris*que sequor *disponere* causas, etc. . .

e. g., V, 509 sqq. of the motion of the stars *three* conjectural explanations, with two alternatives for the third; for the light of the moon 575 *two;* the periodical mutations in the sun's course 614 sqq. *two;* the problem of night (650), *two;* the correlation of day and night 680 sqq. *three;* moon's phases 705 sqq. *three;* eclipses 750 sqq. *two.*

Thunder VI, 96, *nine* explanations; lightning (246), *four;* waterspouts (423), *two;* clouds (451), *five;* rain (495), *four;* earthquakes (535), *four;* rise of Nile (712), *four.*

It is a matter of some interest, philologically, to survey the range of expression in which each writer presents the modality of possibility of alternative conjecture; in Diog. L. 10, 93; *᾽ενδέχεται. . . ὁμοίως. . . ἢ καὶ. . . ἢ καὶ—*; 94 *καὶ ὁμοίως, . . ἔτι δὲ καί. . . ἔτι τε ᾽ενδέχεται. . . ᾽ενδέχεται δὲ. .*˙. in 95; *δύναται καί. . . . καί. . .* in 107; *᾽ενδέχεται. . . γίνοιτο ἂν. . . ᾽αποτέλεσιν ἂν λαμβάνοι. . .* in 111 *ἤτοι. . . ἤτοι ἢ.*—in 112; *οὐ μόνον. . . ᾽αλλὰ καί: . . ἢ. . .—καὶ κατ ἄλλους δὲ πλείονας τρόπους δύναται.* With this compare Lucretius V, 515 sqq. Aut. . . est etiam quoque uti possit. . .; 375 sq. sive. . . . sive. . . .; 637 fit quoque ut; 651 Aut. . . aut. . . quia; also 658, 660, 682, 697; aut etiam quia 701; potest 705, est etiam quare 715; and 731 sqq. cur nequeat. . ., difficilest ratione docere. . . 753 sqq. cur luna queat. . . non posse putetur. . . 762, cur terra queat. . . 765 aliut nequeat. . . and in VI, 97 propterea quia. . . 108 etiam. . . 116 fit quoque. . . ut; 121 hoc etiam pacto. . . videntur; 132 est etiam ratio. . .; 137 fit quoque ut. . . 142 sunt quoque 156 denique. . . 160 item.—295 est etiam cum.

It cannot be my aim to enter into the detail, much less into the scientific merits, of these explanations; it is curious and noteworthy that Seneca in book VI of his *naturales quaestiones* dealing with the problem of earthquakes [a theme suggested by the great earthquake of 63 A. D., from which Pompei and all the gulf of Naples suffered], in reviewing the extant theories on earthquakes, while quoting the Epicurean Metrodorus c. 19; and Epicurus himself does not mention Lucretius, with whom he was familiar. Now Seneca puts Epicurus 6, 20 in the category of those "qui *omnia ista quae retuli* in causa esse dixerunt

aut *ex his plura.* And particularly VI, 20, § 5 is so strong a confirmation of the letter to Pythocles that it seems pertinent to give part of it entire: omnes istas posse esse causas Epicurus ait pluresque alias temptat, et *alios*, qui *aliquid unum* ex iis esse adfirmaverunt, *corripit*, cum sit arduum de his quae coniectura sequenda sunt, aliquid certi promittere." And so the version of Seneca contains the following words or phrases of alternative conjectural statement: potest, potest, fortasse enim, fortasse, fortasse, fortasse, fortasse, fortasse . . . et inde aut, aut.

But Lucretius has further themes which hardly come within the sphere of *μετέωρα*, Etna, Nile, exhalation of Avernus, odd changes of temperature in a certain spring, the Magnet, Epidemics, the Plague at Athens. True, but his fundamental interest is that of ad Pythoclem § 104: *μόνον ὁ μῦθος ἀπέστω, ἀπέσται δὲ, ἐάν τις καλῶς τοῖς φαινομένοις ἀκολουθῶν περὶ τῶν ἀφανῶν σημειῶται.* The absolute elimination of divinity as factor or efficient cause, § 113 and 116, 'learn this by heart,' my dear Pythocles; for the sequence is stated as a two-fold one: *κατὰ πολύ τε γὰρ τοῦ μύθου ἐκβήσῃ καὶ τὰ ὁμογενῆ τούτοις συνορᾶν δυνήσῃ.* And so we see Lucretius engaged in elaborate and ambitious efforts to apply the abstract and fundamental doctrines of atomism, *e. g.*, in dealing with Etna, 647 sqq., with Avernus and its reputed exhalations, 769, 790 sqq., w. the magnet, 906 sqq., where the preliminary elaboration of first principles is carried on with such fulness that the poet apologizes:

> 919. et minimum longis ambagibus est adeundum and
> 1081. nec tibi tam longis opus est ambagibus usquam
> nec me tam multam hic operam consumere par est . . .

and while it is his ambitious attempt to apply fixed principles (cf. Diog. L. 10, 116, "*τὴν τῶν ἀρχῶν καὶ ἀπειρίας καὶ τῶν συγγενῶν τούτοις θεωρίαν*)" to definite physical problems which swelled the theme of the *Magnet* to the bulky total of 184 lines (905–1089), let us glance at the theme of Thunder and Lightning in the earlier part of book VI, 96–379, a little less than 300 lines . . . and then follows the fervid attack on the formulæ of the Etruscan ritual and the folly of ascribing these manifestations to Jupiter;

which uprooting of popular fear of the gods with its interdependence with the fear of death is really the chief motive and the very essence of this unique poem . . . the practical moral interest of emancipating the soul vastly predominates over the didactic or speculative interest.

But the limits of the *liber*, the mechanical necessity even of limitation, so instructively elaborated by Th. Birt in his "Das Antike Buchwesen," 1882, put their constraint upon the poet; so that alongside of these disproportionate elaborations of particular themes as just noted we find, *e.g.*, VI, 527 sqq. snow, winds, hail, hoarfrost, ice merely summarily mentioned, and turned over to the reader's application of first principles. We must not incline, however, to the assumption that this apparent miscellany of physical and meteorological themes and problems in Lucretius V and VI was a mere appendix, or second-thought supplement of the work proper; for in the very first detailed announcement of his chief themes, in I, 127, this entire matter is even placed first in order:

> Qua propter bene cum *superis de rebus* habenda
> nobis est ratio, solis lunæque meatus
> qua fiant ratione. . .

In conclusion we ask were the *μετέωρα* an essential part of the 37 bb. *περὶ φύσεως*? It seems impossible to prove that the letter to Herodotus, § 35, 83, in Diog. L., X, is a true, *i. e.*, an even and truly proportioned summary of the entire range of the great work of 37 bb., the brief reference to *μετέωρα* in § 76 is too slender for elaborate or positive inferences. In the list of E's works Diog. L., 10, 27, of some forty-nine titles with 89 *volumina* are recorded as *τὰ βέλτιστα* out of the total of 300 *κύλινδροι* with the exception of *περὶ νότων δόξαι* there is no title specifically bearing on the subject of *μετέωρα*.

New York University, 1898.

[Annals N. Y. Acad. Sci., XI, No. 22, pp. 443 to 499, January 18, 1899.]

RECORDS OF MEETINGS

OF THE

NEW YORK

ACADEMY OF SCIENCES.

January, 1898, to December, 1898.

RICHARD E. DODGE,

Recording Secretary.

[ANNALS N. Y. ACAD. SCI., XI, No. 22, pp. 445 to 499, January 18, 1899.]

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January, 1898, to December, 1898.

RICHARD E. DODGE, *Recording Secretary.*

REGULAR BUSINESS MEETING.

JANUARY 3, 1898.

Academy met with President Stevenson in the chair. The minutes of the last meeting were read and approved. The Secretary presented for election as Resident Members the names of the following candidates which had been duly approved by the Council.

RESIDENT MEMBERS ELECTED.

F. W. Devoe, 101 Fulton street.
W. G. Dewitt, 88 Nassau street.
E. I. Haines, New Rochelle, N. Y.
Michel M. LeBrun, 8 Mountain avenue, Montclair, N. J.
Charles S. Schultz, Hoboken, N. J.
S. L. H. Ward, 67 Wall street.

The Secretary was authorized to cast a ballot for the names read, and all were declared elected.

The section of Astronomy and Physics then organized.

J. F. KEMP,
Secretary.

SECTION OF ASTRONOMY AND PHYSICS.

January 3, 1898.

Mr. Dudley in the chair, eighteen members and guests present. W. Hallock was appointed Secretary, *pro tem.* Minutes of last meeting read and approved.

The first paper was by **H. Jacoby,** entitled Photographic Researches near the North Pole of the Heavens. Professor Jacoby explained how the "trail plates" are taken with stationary telescope having in its field the north pole point, and pointed out how, after proper corrections, an improved location of the pole could be obtained as the common center of the trail arcs. The results are excellent, and bid fair to give much better values for declination than those obtained by other methods.

The paper was discussed by Professor Rees, Mr. Post and Professor Hallock.

The second paper was by **P. H. Dudley,** entitled The Completion of Relaying the Track of the Boston and Albany Railroad with 95-lb. Rails. Mr. Dudley outlined the introduction of rails of improved material and section, and the gradual relaying of this line, showing how greatly the road was improved at all points, how heavier loads were carried, and how a gain was obtained in all directions. Meeting then adjourned.

W. Hallock,

Secretary of Section, pro tem.

SECTION OF BIOLOGY.

January 10, 1898.

Professor Osborn in the chair, fifty-six persons present. The following programme was offered:

H. F. Osborn, The Origin of the Mammalia.

F. M. Chapman, The Distribution of Birds in the State of Vera Cruz.

F. E. Lloyd, On Hypertrophied Leaf-Scales in Pinus ponderosa.

Professor **Osborn** showed that the speculation of recent authors (Cope, Baur, Osborn) regarding the ancestry of the mammalia turns back to certain Permian reptiles of the orders Theriodontia Owen, and Gomphodontia Seeley. He reviewed the characters of the skeleton of these Theriodontia, showing their unmistakable promammalian features. A number of persistent reptilian characters were also cited. In conclusion, the speaker said that these Theriodontia have the geological age required for ancestors of the mammalia, and are the only type of reptiles which exhibit mammalian affinities. Their great size and certain adaptive specializations alone bar any known type from direct ancestry of the much smaller earliest mammals; but this fact does not preclude the existence of very small unspecialized forms which may have developed into the mammalian type. Professor Osborn's paper was illustrated by lantern slides.

Dr. **Chapman** described the various types of vegetation and the altitudinal distribution of birds along the course of the two railroads running from the coast at Vera Cruz into the tablelands of the interior. His paper was also illustrated by lantern slides. In answer to Professor Britton's question whether the variations in air pressure have any influence in modifying bird structure, the speaker said apparently not. They undergo different pressure, as shown by height of flight, and seem to thrive equally well under differing conditions of barometric pressure.

Professor **Lloyd** showed that scales which subtend the fascicles of *Pinus ponderosa* are morphologically equivalent to leaves; and, when hypertrophied, these leaves closely resemble the leaf of the genus *Pseudotsuga*. The speaker suggested that the Pines may have been derived phylogenetically from a generalized form represented by *Pseudotsuga*, and that the hypertrophied leaves are atavistic.

GARY N. CALKINS,
Secretary of Section.

SECTION OF GEOLOGY.

January 17, 1898.

Professor Kemp in the chair, fifteen persons present.

The first paper was by Mr. **Arthur Hollick,** entitled Further Notes on Block Island Geology and Botany. The speaker gave a summary of his work done on Block Island in July, 1897, and particularly of his success in tracing eastward from Long Island the Amboy clays which had previously been determined by paleontological evidence on Staten Island, Long Island and Martha's Vineyard. Something like fifteen species of Middle Cretaceous flora, nine of them typical of the Amboy clays, have been found.

Mr. Hollick then classified the existing flora of the Island physiographically into that of the hills, peat bogs, sand dunes and beaches, salt marshes and salt water. In the course of his work he has added to the already published lists something like twenty-four new species, although it is not considered that this by any means completes the list of possible species that might be found in the springtime. The flora as a whole is distinctly that of a morainal country, and its nearest analogue is that of Montauk Point.

Mr. Hollick then offered some suggestions to account for the present peculiar flora of the island, and particularly for the absence of certain species that would be expected, and showed that two elements are to be taken into consideration, the geological and the human. Block Island is the only part of the terminal moraine along the New England coast which does not have accompanying the moraine a certain amount of plain land which would naturally allow a variety in the flora. It is presumable that Block Island also has been practically separated from the rest of the continent by a deep channel of more than twenty fathoms for a considerable time, and that even before the last depression of the land the island was connected with the mainland merely by a small peninsula. Hence the diversity of the flora as compared with the continent, because of the length of separation.

The speaker also mentioned the extensive archeological discoveries on the west shore of the island, and gave a list of the shells and implements discovered in several of the kitchen middens, and also of the bones of animals brought to light in the old fireplaces in the sand dunes. He made particular mention also of the great numbers of *Littorina*, the common periwinkle of Europe, which has never before been announced from Block Island. The paper was discussed by Professor Lloyd and Dr. Martin.

The second paper of the evening was by **Richard E. Dodge,** entitled SCIENTIFIC GEOGRAPHY IN EDUCATION. The speaker brought out the point that geography work may be classified into three divisions, that for the common schools, the secondary schools, and the universities, and outlined briefly a few suggestions as to how the subject matter might be treated scientifically in each of the groups, and the dependence of each group upon the others. He paid particular attention to the difficulties of securing scientific work in geography in the grade schools, and to the fact that geography at present is extremely unsatisfactory in most of our schools, probably because of the lack of inspiration owing to the neglect of the subject hitherto in the universities of the country. The paper was illustrated by the exhibition of cheap and easily procurable maps, that can be used for scientific geography work of several grades.

The meeting then closed with a few remarks by Professor **Kemp,** in reference to the famous classic, entitled LITHOGRAPHIÆ WIRCENBURGENSIS DUCENTIS LAPIDUM FIGURANTORUM, A POTIORI INSECTIFORMIUM PRODIGIOSIS IMAGINIBUS EXORNATÆ, SPECIMEN PRIMUM, written by J. B. A. Beringer in 1726. Professor Kemp summarized the work of the author in attempting to explain a great collection of pseudo fossils from a theological standpoint, the fossils having previously been made by some practical jokers and buried in the rocks for the author to find.

RICHARD E. DODGE,
Secretary of Section.

SECTION OF PSYCHOLOGY AND ANTHROPOLOGY.

JANUARY 24, 1898.

Professor Bliss in the chair. Fourteen names proposed for membership by the Secretary, were referred to the Council.

The following papers were then presented:

E. L. Thorndike, EXPERIMENTS IN COMPARATIVE PSYCHOLOGY.

H. J. Smith, RECENT ARCHEOLOGICAL INVESTIGATIONS IN BRITISH COLUMBIA.

L. Farrand, REPORT OF THE MEETING OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION AT ITHACA.

CHARLES B. BLISS,
Secretary of Section.

REGULAR PUBLIC LECTURE.

JANUARY 31, 1898.

The Academy met in the Mott Memorial Library and listened to the second public lecture of the season, which was delivered by Professor **Henry H. Rusby,** of the College of Pharmacy, upon the subject, AN AFTERNOON ON A VENEZUELAN BAYOU.

Thirty persons were present and at the conclusion a vote thanking the speaker was passed.

J. F. KEMP,
Secretary.

REGULAR BUSINESS MEETING.

FEBRUARY 7, 1898.

The Academy met, with President Stevenson in the chair. About twenty-five members present. The minutes of the last meeting were read and approved. The Secretary presented the following nominations of new members from the Council:

Corresponding Member Elected.

Professor George E. Hale, Yerkes Observatory, Williams Bay, Wis.

Resident Members Elected.

James Boyd, 12 Franklin street.
Alfred S. Brown, 160 West 76th street.
William Phelps Eno, 111 Broadway.
William W. Hoppin, 111 Broadway.
J. Morgan Howe, M.D., 58 West 47th street.
John S. Kennedy, 6 West 57th street.
Solomon Loeb, 37 East 38th street.
Edwin S. Marston, 20 William street.
George L. Nichols, 66 East 56th street.
Wheeler H. Peckham, 685 Madison avenue.
J. Hambden Robb, 23 Park avenue.
Henry H. Rogers, 26 East 57th street.
J. A. Roosevelt, 4 West 57th street.
H. L. Thornell, 51 West 73d street.
Spencer Trask, 27 Pine street.
John I. Waterbury, Morristown, New Jersey.
Frederick H. Wiggin, 55 West 36th street.
Alfred R. Wolff, 15 West 89th street.
C. A. Woodward, D.D.S., 49 West 36th street.
George Zabriskie, 45 West 48th street.

On motion the Secretary was instructed to cast a ballot for all the nominees, which was done. The Secretary presented the following recommendation from the Council, which, on motion, was adopted by ballot :

Resolved, That in consideration of the extremely valuable and conscientious services to the Academy of Professor D. S. Martin, his past dues be hereby remitted, and that he be made a Life Member.

Amendments to By-Laws.

The Secretary laid before the Academy the following amendments to the by-laws, which had been recommended by the Council :

Chapter I, to add Article 4: "The number of Fellows shall be limited to 100."

Chapter V, Article 1, to add: "Past Presidents of the Academy, residents of New York City, shall be advisory members of the Council, with a right to be present at the meetings and to serve on committees, but without vote. They shall be notified of all meetings."

The Section of Astronomy and Physics then organized.

JAMES F. KEMP,
Secretary.

SECTION OF ASTRONOMY AND PHYSICS.

FEBRUARY 7, 1898.

The meeting was called to order with Mr. P. H. Dudley in the chair, twenty-one members and guests being present. The reading of the minutes of the last meeting was omitted, and the section proceeded with the programme of the evening.

H. Jacoby took the chair; and **P. H. Dudley** read a paper, illustrated by lantern views, entitled THE USE OF THE DUDLEY STREMMATOGRAPH FOR DETERMINING THE STRAINS PRODUCED IN RAILS BY MOVING TRAINS." He described the use of the instrument in recording tensional and compressive stresses in steel rails under various kinds of traffic, and stated that much greater stresses exist in steel rails than are commonly supposed to be caused by locomotives and cars standing on or moving over them. After a few supplementary remarks in reply to questions, Mr. Dudley resumed the chair, and **W. S. Day** read a paper entitled RECENT EXPERIMENTS CONCERNING THE SPECIFIC HEAT OF WATER. He discussed the results obtained by Rowland, Schuster, Bartoli, Griffiths and Miculescu, in measuring the mechanical equivalent of heat, and compared the results obtained by these scientists by means of curves. The paper was discussed by W. Hallock, H. Jacoby and others. After a few concluding remarks by Professor Jacoby, the meeting adjourned.

R. GORDON,
Secretary of Section.

SECTION OF BIOLOGY.

February 14, 1898.

Professor Osborn in the chair. Twenty-one persons present. The following programme was presented:

George S. Huntington, The Eparterial Bronchial System of the Mammalia.

F. S. Lee, The Function of the Ear and Lateral Line in Fishes.

Professor **Huntington's** paper dealt with the structure of the Bronchial System and with the pulmonary supply in various representatives of orders and families of the Mammalia. The conclusions reached are at variance, in their main points at least, with the views expressed by Professor Achy and with the generally accepted views in the text-books of human and comparative anatomy. The most primitive form appears to be Achy's "bilateral hyparterial type," represented by Achy in *Hystrix cristata*, by Weber in *Balæna mysticetus*, and now by the author in *Taxidea Americana*.

In the other mammalia a distinct and progressive series can be established between the primitive types of bronchial distribution and the most complex arrangement.

Among the many conclusions reached by Professor Huntington, we may note the following: The active agent in changing the architecture of the lung is not the pulmonary artery (Achy), but the migration of the cephalic primary trunk or its proximal secondary derivative for increasing respiratory area. The pulmonary artery, in the majority of forms, is lateral; hence, distinction in "dorsal" and "ventral" should be abandoned, etc.

Dr. Huntington's paper was well illustrated by lantern slides.

Dr. **Lee,** after describing his experiments on the auditory functions of certain fish, came to the following conclusions: (1) the otolithic organs mediate the perception of progressive movement; (2) all experiments for demonstrating the power of hearing in the customary sense, have failed, but destruction of the organs of the lateral line, combined with removal of the large pectoral and ventral fins in some species (*Batrachus tau*) causes

lack of appreciation of equilibrium, also central stimulation of lateral nerve causes coördinated compensating movements of the fins exactly similar to those caused by stimulation of the acoustic nerve. The inference then is that the organs of the lateral line are sense organs of equilibrium analogous to the ear; (3) the ear is a derivative of the lateral line.

Dr. Lee's paper was illustrated by models, charts and diagrams.

Dr. J. A. Blake was nominated for membership, and referred to Council.

GARY N. CALKINS,
Secretary of Section.

STATED MEETING.

FEBRUARY 21, 1898.

The Academy met with President Stevenson in the chair. Seventeen members present.

The Secretary presented the following nominations for resident membership:

Robert F. Cornish, 123 Claremont avenue, Montclair, N. J.

Mrs. Henry Draper, 271 Madison avenue, New York.

Rev. Dr. Henry Mitchell McCracken, D.D., LL.D., New York University.

Mr. G. F. Kunz presented a circular relating to the proposed dinner of the Scientific Alliance and urged all the members of the Academy to be present.

SECTION OF GEOLOGY AND MINERALOGY.

FEBRUARY 21, 1898.

Professor Kemp in the chair. In the absence of the Secretary, Mr. Gilbert van Ingen was elected Secretary, *pro tem.*

The first paper was by **George F. Kunz,** entitled A RECENT DISCOVERY OF HUGE QUARTZ CRYSTALS IN THE WEST. The

crystals were found in the neighborhood of Grass Valley, Cal., in placer gold mines and, although somewhat waterworn, are reported to be of great size and clearness. One is said to weigh a ton. The paper was discussed by Messrs. Levison and Kemp.

The second paper related to the exhibition of recent accessions of rare minerals, loaned for the purpose, by Professor **A. J. Moses.** Among the rest a large specimen of cellular rock with coats of Huantahajite, the whole being 8 inches square, was of particular interest. In the absence of Professor Moses the specimens were commented on by the Chairman and by Professor Chester.

The third paper was by Professor **F. D. Chester,** entitled KRENNERITE FROM CRIPPLE CREEK, COL. The speaker remarked on the rarity of the mineral and described his good fortune in obtaining a specimen with crystals capable of being measured, which were now being studied by Professor Penfield of Yale. The paper led to a considerable discussion upon the occurrence of the telluride ores, by Messrs. Caswell, Chester, Kunz and Kemp.

Professor **Kemp** then exhibited some specimens of the Nepheline Syenite from Dungannon, Ont., which he had received from Mr. F. J. Pope, and which showed crystals of Corundum of large size, forming an original mineral in the rock.

Dr. **W. G. Levison** exhibited some microscopic mounts of minerals in small pasteboard boxes.

A paper by **Stuart Weller,** entitled DESCRIPTION OF DEVONIAN CRINOIDS AND BLASTOIDS FROM MILWAUKEE, WIS., was read by title.

On motion the meeting adjourned.

GILBERT VAN INGEN,
Secretary, pro tem.

ANNUAL BUSINESS MEETING.

FEBRUARY, 21, 1898.

The Academy met with President Stevenson in the chair. Fifty persons present. There being no minutes to read, the President called for the Annual Reports of the various officers.

REPORT OF THE RECORDING SECRETARY.

The year now closing has been a successful and gratifying one in the history of the Academy. The meetings have been well attended, the quality of the papers good, and the general interest in the affairs of the organization has been pronounced. The membership has increased beyond any previous figure in its history.

There have been nine meetings of the Council, nine regular business meetings of the Academy, twenty-two additional stated meetings, five public lectures, one public reception, and two receptions to distinguished scientific visitors from abroad.

The Section of Astronomy and Physics has held eight meetings, with an average attendance of twenty; the Section of Biology has held eight, with an average attendance of twenty-four; the Section of Geology and Mineralogy eight, with an average of thirty-three; the Sub-section of Philology three, averaging twenty-seven; and the Sub-section of Psychology and Anthropology four, with about the same number.

A total of eighty-three papers has been presented, not including Public Lectures, which may be classified as follows:

Anatomy	1	Geology	16
Anthropology	5	Mechanics	1
Archeology	2	Mineralogy	4
Astronomy	6	Paleontology	8
Botany	1	Philology	5
Civil Engineering	1	Physics	9
Chemistry	4	Psychology	2
Geography	3	Zoölogy	18

Fifty-one new members have been elected, nine have resigned, and three have died, leaving a total of 330 on the Secretary's list, a gain of 39 over last year. As stated above, the resident membership of the Academy is now the largest in its history. One Honorary Member has been elected and ten nominations are pending. One Corresponding Member has been elected and the nominations of fifteen are pending. Nineteen Resident Members have been elected Fellows.

In connection with the publications the Council has decided that it is inadvisable to issue two octavo series and a quarto. The *Transactions* will therefore be discontinued with Volume XVI and will be merged into the ANNALS, beginning Volume XI of the latter. While the same size of page will be preserved a new and more desirable font of type has been chosen. The records of the meetings will be printed separately from the scientific papers. The volumes will also run coincidently with the calendar years.

The by-laws have been amended so as to abolish the fee for election as Fellow. And so as to limit the number of Fellows to one hundred.

The public reception of the Academy in March last passed off most successfully and the exhibition has now become an annual event, anticipated both within and without the Academy.

The Academy extended to Sir Archibald Geikie in May last a reception which proved a very enjoyable occasion; and in October offered its hospitalities to Dr. Albrecht Penck.

Respectfully submitted,
J. F. KEMP,
Recording Secretary.

ANNUAL REPORT OF THE TREASURER.

RECEIPTS.

Balance on hand as per last Annual Report.......	$394.89
Contribution to Audubon Fund	100.00
Income of Audubon Fund.....................	89.86

Income of Publication Fund		$60.00
" Permanent Fund		300.14
Life Membership Fee		100.00
Initiation Fees		250.00
Annual Dues, 1894	$10.00	
" " 1895	30.00	
" " 1896	80.00	
" " 1897	2,035.00	
" " 1898	290.00	2,445.00
Proceeds Sale of Electrical Fixtures		20.00
		$3,759.89

DISBURSEMENTS.

Net Cost of Publishing Annals	$690.14
Net Cost of Publishing Transactions	1,033.71
Expenses of Recording Secretary	380.68
" Secretary of Biological Section	15.22
" Librarian	82.46
Cost of Accession to Library	14.13
Expenses of Treasurer	31.63
Janitorial Services	48.00
Rent of Rooms, Oct. 1, 1897, to Jan. 1, 1898	70.00
Insurance Premium	19.37
Expenses of Lectures	58.00
" Fourth Annual Reception	546.52
" Moving	32.63
	$3,022.49
Balance, Cash now on hand	737.40

DETAILS OF PERMANENT FUND.

Balance on hand as per last Annual Report	$348.68
Life Membership Fee received during the year	100.00
Initiation Fees received during the year	250.00
Balance now on hand	$698.68

Details of Audubon Fund.

Contribution received from Mrs. Esther Hermann. .		$100.00
Income from Bond and Mortgage Investment		89.86
Charged back to Publication Fund.		23.00
Balance now on hand.		$212.86

Details of Publication Fund.

Income from Bond and Mortgage Investment		$60.00
Credited back to Audubon Fund.	$23.00	
Credited to General Income a/c	37.00	
	$60.00	

Details of General Income Account.

Balance brought down as per last Annual Report. .		$46.21
Income from Permanent Fund		300.14
" " Publication Fund.		37.00
Proceeds Sale of Electrical Fixtures.		20.00
Received for Annual Dues		2,445.00
		$2,848.35
Less :		
Net Cost of Publishing Annals and Transactions. . .		$1,723.85
Expenses of Officers		509.99
Rent of Rooms, Janitorial Services and Cost of Moving.		150.63
Cost of Accessions to Library	$14.13	
Cost of Lectures.	58.00	
Cost of Fourth Annual Reception.	546.52	
Insurance Premiums.	19.37	$3,022.49
Deficiency in Income to meet Current Expenses. . .		174.14

Summary.

Balance to credit of Permanent Fund	$698.68
" " Audubon Fund	212.86
	$911.54
Less Deficit in General Income.	174.14
Balance, Cash on hand	$737.40

ASSETS.

Cash in Bank		$737.40
Investments in Bonds and Mortgages,		
a/c Permanent Fund	$8,402.75	
a/c Publication Fund	1,800.00	
a/c Audubon Fund	1,797.25	$12,000.00
Annual Dues, in arrears,		
For 1895	20.00	
" 1896	140.00	
" 1897	310.00	470.00
Total		$13,207.40
As against amount last year		12,644.89

Respectfully submitted,

C. F. Cox,

Treasurer.

On motion the report was referred to the Finance Committee for auditing.

HONORARY MEMBERS ELECTED.

The following nominations of Honorary Members were then presented from the Council:

Professor Arthur Anweers, Berlin.

Professor W. K. Brooks, Johns Hopkins University, Baltimore.

Dr. David Gill, Astronomical Observatory, Cape of Good Hope.

Dr. George W. Hill, Nyack, N. Y.

Professor E. Ray Lankester, Oxford, England.

Professor Albrecht Penck, Vienna, Austria.

Professor W. Pfeffer, Leipzig, Germany.

Professor Hans Reusch, Christiania, Norway.

Professor Karl von Zittel, Munich, Germany.

Professor R. Virchow, Berlin.

On motion, the Secretary was instructed to cast a ballot for all the nominees, which was done and they were declared elected.

Corresponding Members Elected.

The following nominations of Corresponding Members were presented from the Council:

Professor F. D. Adams, Montreal.
Professor W. B. Balfour, Edinboro, Scotland.
Professor George Baur, Chicago.
Dr. William Carruthers, British Museum, London.
Professor T. C. Chamberlin, Chicago.
Professor William M. Davis, Cambridge, Mass.
Dr. A. Franchet, Paris.
Professor J. P. Iddings, Chicago.
Professor C. S. Minot, Boston.
Dr. George Murray, British Museum, London.
Professor W. B. Scott, Princeton, N. J.
Professor C. O. Whitman, Chicago.
Professor H. S. Williams, New Haven.
Mr. C. D. Walcott, Washington.

On motion the Secretary was instructed to cast a ballot for all the nominees, and they were declared elected.

Election of Officers.

The Academy then proceeded to the election of officers for the ensuing year. The following ticket was elected by ballot:

President, Henry F. Osborn.
1st Vice-President, Nathaniel L. Britton.
2d Vice-President, James F. Kemp.
Corresponding Secretary, William Stratford.
Recording Secretary, Richard E. Dodge.
Treasurer, Charles F. Cox.
Librarian, Arthur Hollick.

Councilors, Charles L. Bristol, Bashford Dean, Charles A. Doremus,, William Hallock, Harold Jacoby, Lawrence A. McLouth.

Curators, Harrison G. Dyar, Alexis A. Julien, George F. Kunz, Louis H. Laudy, William D. Schoonmaker.

Finance Committee, Henry Dudley, John H. Hinton, Cornelius Van Brunt.

Annual Address of retiring President, J. J. Stevenson.

On the announcement of the election, President Osborn took the chair and assumed control of the meeting. Retiring President **Stevenson** then delivered the annual Presidential Address upon the subject, The Debt of the World to Pure Science. The address appears in the Annals, Vol. XI, pp. 177–192. At the conclusion of the address, President Osborn expressed the thanks of the Academy to the speaker, and the meeting adjourned.

REGULAR BUSINESS MEETING.

March 7, 1898.

President Osborn in the chair, fifteen members present. Minutes of last meeting read and approved.

The amendments to the by-laws proposed at the February meeting were both carried.

The Secretary presented for the Council the following names for Resident Membership, and he was authorized to cast one ballot for the list, which was done.

Resident Members Elected.

Robert H. Cornish, 123 Claremont avenue, Montclair, N. J.

Henry Mitchell MacCracken, D.D., LL.D., New York University.

Dr. Joseph A. Blake, 437 West 59th street.

Mrs. M. A. P. Draper, 271 Madison avenue.

Members Proposed.

The following nominations were read and referred to the council:

Life member, Miss Catherine W. Bruce, 810 Fifth avenue; nominated by J. K. Rees.

Resident members, S. B. Hoffman, Morristown, N. J. nominated by Harold Jacoby.

Douglass Burnett, 42 Livingston street, Brooklyn, N. Y.; nominated by P. H. Dudley.

The following paper was read by title, and referred to Publication Committee: THE NORTHROP COLLECTION OF CRUSTACEA, by Professor **Walter M. Rankin,** of Princeton, illustrated by three plates.

The Section of Astronomy and Physics then organized.

RICHARD E. DODGE,
Secretary.

SECTION OF ASTRONOMY AND PHYSICS.

MARCH 7, 1898.

Meeting was called to order by the Chairman, Mr. P. H. Dudley, there being eighteen persons present. The first business of the meeting was the election of officers for the ensuing year. Nominations being declared open, J. K. Rees nominated P. H. Dudley as Chairman. There being no other nominations, the Secretary was empowered to cast one ballot for Mr. Dudley; and he was declared elected Chairman. R. Gordon was nominated Secretary by W. Hallock. There being no other nominations, the Secretary was empowered to cast one ballot for him; and he was thereby declared elected.

The next business was the reading of papers.

Romeyn Hitchcock read a paper entitled, INDUSTRIAL APPLICATIONS OF OXYGEN in which he described a gas enriched by oxygen for the purpose of increasing its heating power. He compared the composition of this gas, with which experiments have been carried on recently, with that of several of the usual gases commercially employed for lighting and heating. After brief discussion, **W. Hallock** described a MAKE-CIRCUIT PENDULUM, and showed a working model of the same. After some discussion, the meeting adjourned at 9:20 P. M.

R. GORDON,
Secretary of Section.

STATED MEETING.

MARCH 14, 1898.

President Osborn in the chair.

Secretary read the following nominations of committees made by the President, from the body of the Council for the ensuing year.

Committee on Publication: President and Secretary, Professors Dean, Jacoby, McLouth, Kemp and Britton.

As *representatives of the New York Academy of Sciences in the Scientific Alliance:* The President, Professor Stevenson and Mr. Cox.

Secretary then made announcement of the proposed grant of the Newberry Fund for the ensuing year.

Section of Biology then formed.

RICHARD E. DODGE,
Secretary.

SECTION OF BIOLOGY.

MARCH 14, 1898.

Professor Wilson in the Chair. Twenty-three persons present. The following program was offered:

1. **B. B. Griffin,** A DESCRIPTION OF SOME MARINE NEMERTEANS FROM PUGET SOUND AND ALASKA.

2. **W. H. Hornaday,** THE DESTRUCTION OF BIRDS IN THE UNITED STATES.

3. **N. R. Harrington,** REPORT ON THE CRUSTACEA OF PUGET SOUND.

4. **H. E. Crampton, Jr.,** AN IMPORTANT INSTANCE OF INSECT COALESCENCE.

In the absence of the author, Mr. **Griffin's** paper was read by title.

Mr. **Hornaday** first described the method employed to reduce bird loss to figures. Circulars containing the following questions were sent out to trappers, guides, sportsmen and naturalists in all parts of the United States. (1) Are birds decreasing

in your locality? (2) How many birds are there now compared with fifteen years ago? (3) What are the most destructive agents? (4) Are any birds becoming extinct? The answers came from all but four States and territories, and showed surprising agreement. The most destructive agencies are sportsmen, plume-hunters, boys after eggs, pot-hunters, fire, English sparrows, etc.; and through these it has been estimated that there has been a decrease of about 46% during the last fifteen years. It was shown that game and edible birds are becoming scarce, and that song birds are being used for food in their stead; that plume-birds are becoming extinct, and that destructive agencies are increasing. Mr. Hornaday concluded with an appeal for more drastic measures in our game laws and for their careful execution. The paper was discussed by the Chairman, by Professor Osborn and by Mr. Mathews.

Mr. **Harrington's** report was based on the Crustacea collected at Puget Sound in 1896, and worked up by W. T. Calman, University College, Dundee, Scotland. It dealt with sixty-three species, some three of which were new. One of the new species, a parasite, *Pseudione giardi*, is interesting because males, female, and larva, were all found on a single specimen of its host *Eupagurus ochotensis;* another new species, *Polycheria osborni* is interesting because the only other known representative of the genus is found in the Antarctic region. The entire collection was made up as follows: *Macrura*, 15 species (13 of which were shrimps); *Brachyura*, 34 species; *Isopoda*, 6 species; *Amphipoda*, 3 species; Copepoda, 1 species.

Mr. **Crampton** spoke of his experiments on insect-grafting, and of one case in particular where the colors of the scales of one species were imposed upon the scales of another. The paper was discussed by Dr. Dyar and others.

The Secretary of the Academy notified the Section that the income of the John Strong Newberry Fund of the Council of the Scientific Alliance is to be awarded this year to a paleontologist or a zoölogist; and that a candidate should be chosen before the Council meeting of April 2d.

GARY N. CALKINS,
Secretary of Section.

STATED MEETING.

MARCH 21, 1898.

President Osborn in the chair. Minutes of meeting of February were read and approved. Secretary read the following paper by title:

THE PHYSIOLOGY OF SECRETION, by **Albert P. Mathews.**

Section of Geology and Mineralogy then formed.

RICHARD E. DODGE,
Secretary,

SECTION OF GEOLOGY AND MINERALOGY.

MARCH 21, 1898.

Professor Kemp in the chair. Thirty-four members present. Minutes of the last meeting were read and approved. Secretary read a letter from the Secretary of the Scientific Alliance in reference to the Newberry grant for paleontology or zoölogy.

The paper of the evening, illustrated by lantern, was by Dr. **Heinrich Ries,** entitled THE CLAY AND KAOLIN DEPOSITS OF EUROPE. Dr. Ries sketched briefly the geographical distribution of the Kaolin deposits, and their relation and comparison to similar deposits of America. He then gave special attention to the deposits of Great Britain, Belgium, Denmark, Germany and Austria, and mentioned briefly those found in other regions He described particularly the deposits of Cornwall, which are found in association with veins of Tin and Granite in regions where it is supposed that the Feldspar has been changed to Kaolin through the influence of fluoric fumes rising from below. These products are very pure, containing 97½% of clay substance. He also spoke of the ball plastic clays found in southwestern England, which occur in lenses in large beds of sand, and are used to mix with non-plastic kaolins. Refractory clays are found in England and Scotland in the Carboniferous rocks, and are worked by underground mining. Impure clays, used for bricks, are particularly found in the vicinity of London. The Staffordshire

blue brick, Fuller's earth and Bath brick deposits were sketched briefly, and the technological treatment in Great Britain, Germany and the United States was compared. The latter part of the paper was devoted to a rapid summary of the position, quality, uses and manner of mining of the famous clays of Bornholm, Denmark; of the Glasspot clays of southeastern Belgium; of the Kaolin deposits of Limoges, France, and the deposits of Prussia.

The paper was discussed by Dr. Julien, the Chairman, and Professor Hallock.

Professor **Henry F. Osborn** described the progress this year made through international effort in correlating the larger divisions of the fresh water Tertiary deposits of Europe by a study of the vertebrate remains.

Professor J. F. Kemp was nominated for *Chairman of the Section* for the ensuing year. There being no other nomination he was unanimously elected.

Dr. Heinrich Ries was nominated for *Secretary of the Section* and unanimously elected.

Academy adjourned at 9:15.

RICHARD E. DODGE,
Secretary of Section.

SUB-SECTION OF PHILOLOGY.

MARCH 28, 1898.

Meeting called to order by Chairman, Professor T. R. Price.

Officers for the ensuing year were elected: Lawrence A. McLouth, *Chairman*; A. V. Williams Jackson, *Secretary*.

Moved and carried to request Council of Academy to provide for four meetings of Philological Section for 1898–99.

The following papers were read and discussed:

E. G. Sihler, THE LATTER PART OF LUCRETIUS, AND EPICURUS *περί μετεώρων*.

J. R. Wheeler, THE NEWLY DISCOVERED POEMS OF BACCHYLIDES.

B. D. Woodward, The Vowels of the Rumanian and other Romance Languages.

On account of the lateness of the hour, the reading of the last paper on the programme was postponed.

Lawrence A. McLouth,
Secretary of Section.

REGULAR BUSINESS MEETING.

April 4, 1898.

Academy met at 8:10, President H. F. Osborn in the chair.

Minutes of the last meeting were read and approved.

Secretary submitted the following list of names that had been approved by the Council for election, and was authorized to cast one ballot for the same, which was done.

Members Elected.

Miss Catherine W. Bruce, 810 Fifth avenue, Life Member

Douglas Burnett, 42 Livingston street, Brooklyn.

S. V. Hoffman, Morristown, N. J.

Members Proposed.

The following candidates for membership were read and referred to the Council under the rules:

Francisco G. P. Leão, Chancellor of the Brazilian Consulate.

C. E. Tripler, 121 West 89th street.

Dr. L. T. Chamberlain, 128 Fifth avenue.

The President appointed Mr. P. H. Dudley as the *representative of the Academy in the Scientific Alliance*, in the place of Professor J. J. Stevenson, resigned.

The Secretary made announcement of the changes to be incorporated in the eleventh volume of the Annals, now under way, with certain new regulations in reference to the printing of papers, and gave a statement of recent business transacted by the Council.

Section of Astronomy and Physics then organized.

Richard E. Dodge,
Secretary.

SECTION OF ASTRONOMY AND PHYSICS.

April 4, 1898.

The Section organized with Mr. P. H. Dudley, the Chairman, presiding. There were sixteen persons present. After the reading of the minutes of the last meeting, the following papers were presented:

Mr. **W. G. Levison** showed a Photographed Eye-Piece Micrometer, and described the construction of it, also speaking of micro-organisms as a complication in washing photographic plates. He, in addition to this, described and showed a model of a simple phosphoroscope. The discussion on these subjects was participated in by W. Hallock, C. F. Cox, H. F. Osborn, and C. C. Trowbridge.

The next paper was entitled A Modification of Mance's Method of Measuring Battery Resistance, by **W. S. Day.** This was treated mathematically by Dr. Day at considerable length. After this **Frank Schlesinger** read a short paper upon The Præsepe Group, mentioning the measurement and reduction of the Rutherford photographs of this group. After a few questions by members on the subject of the measurements the meeting adjourned.

Reginald Gordon,
Secretary of Section.

SECTION OF BIOLOGY.

April 11, 1898.

Professor Wilson in the chair. Eighteen persons present. Election of sectional officers for ensuing year. Dr. Dean seconded by Professor Stratford, moved that Professor Wilson and Mr. Calkins be reëlected to their respective offices, and the Secretary was directed to cast one affirmative ballot.

The following programme was announced:

1. **O. S. Strong,** A New Point on the Innervation of the Lateral Line Organs.

2. **A. P. Mathews,** THE PHYSIOLOGY OF SECRETION.
3. **G. N. Calkins,** THE ORIGIN OF PROTOZOAN NUCLEI.
4. **F. C. Paulmier,** SPERMATOGENESIS IN HEMIPTERA.

Dr. Strong explained some exceptions which have been urged to the view that the lateral line organs are innervated exclusively by special roots having a common center in the medulla. Among these exceptions is the innervation of a certain canal-organ by a branch of the glossopharyngeus instead of by a lateral line nerve proper. Dr. Strong showed that, close to the medulla in the young dog-fish (*Squalus acanthias*) a small intracranial bundle of fibers becomes detached from the lateral line root, and fuses with the glossopharyngeus. This bundle retains its identity, shown by greater calibre, etc. On emerging from the auditory capsule the bundle becomes detached and passes to a canal organ. Similar fibers described by Kingsbury in *Amia*, would probably be found to have the same history.

The three other papers were read by title, the authors not being present.

H. E. CRAMPTON,
Secretary of Section, pro tem.

FIFTH ANNUAL EXHIBITION.

APRIL 13 AND 14, 1898.

The Fifth Annual Reception was held in the American Museum of Natural History, April 13 and 14, 1898, under the control of the following committee, assisted by the chairmen of fifteen departments of science: Henry F. Osborn, Reginald Gordon, Charles F. Cox, Gary N. Calkins, and Richard E. Dodge, Chairman.

The exhibition lasted two evenings and one afternoon, and was attended by an estimated number of more than 6,000 people. The annual lecture was given April 14th by Professor **George E. Hale,** of Yerkes Observatory, on "THE FUNCTION OF LARGE TELESCOPES." Several demonstrations of Liquid Air were given by Mr. Charles E. Tripler.

The programme and catalogue of this exhibition is printed as an appendix to Part I of Vol. XI of the ANNALS.

RICHARD E. DODGE,
Secretary.

STATED MEETING.

APRIL 18, 1898.

Academy met with Vice-President Kemp in the chair, in Schermerhorn Hall, Columbia University.

Minutes of the meeting for March were read and approved.

Letters of thanks from Professor J. P. Iddings and Frank P. Adams, accepting the honor of being elected Corresponding Members were read.

Having no further business, the Section of Geology and Mineralogy then formed.

RICHARD E. DODGE,
Secretary.

SECTION OF GEOLOGY AND MINERALOGY.

APRIL 18, 1898.

Professor Kemp in the chair. Thirty-five members present.

Professor Kemp made a few opening remarks and was followed by Dr. **A. A. Julien** who read a paper entitled THE ELEMENTS OF STRENGTH AND WEAKNESS IN BUILDING STONES.

Dr. Julien called attention to the fact that in the testing of building stones little consideration is given to the causes influencing their various properties. In judging the resistance which a stone shows towards weathering, care should be taken to recognize the character of the forces to which it has been subjected. The strength of a stone bears no relation to its mineral components, but is dependent on the shape and arrangement of the mineral grains and the character of the cementing material. In considering the strength of a stone four facts have to be kept in mind, viz.: interlockment of the particles; coherence, de-

pendent on the character of the cement and adhesion of the grains; rigidity and tension. The "quarry sap," Dr. Julien believes, plays a more important rôle than has hitherto been recognized, as it probably carries much of the cement in solution and deposits it only when the stone is exposed to the air. This accounts for the hardening of the stones after being quarried. A distinction should also be made between porosity due to cavities between the grains and interstices in the individual minerals. The former is a source of weakness, the latter not, although either may cause the rock to exhibit a high absorptive capacity. All of these points, which have an important bearing on the strength of building stones are best studied with the microscope. The paper was illustrated by means of sections thrown on the screen with a polarizing lantern. Discussion was by Professor Kemp and Mrs. Dudley.

The second paper of the evening was by **J. D. Irving** on CONTACT-METAMORPHISM OF THE PALISADES DIABASE.

Mr. Irving referred to the work done by Professor Osann and Andrae some years ago and stated that his results agreed with theirs, but recent railroad excavations at Shadyside had enabled him to obtain additional facts. The Diabase flow becomes denser, finer grained and porphyritic towards the contact, with a decrease of Hypersthene. In addition to zones found by Osann, Mr. Irving found: 1. A normal hornfels zone rich in Spinel. 2. A hornfels zone with brown basaltic hornblende layers. 3. Hornfels with an undeterminable isotropic mineral resembling Leucite. 4. Hornfels with Andalusite, gradually changing to Arkose farther from the contact. The Diabase is to be considered as an intruded mass and not a surface flow. The paper was discussed by Professors Kemp, Dodge, Dr. Hovey and Mr. White.

Owing to the lateness of the hour the reading of the other two papers on the programme was deferred until the next meeting.

Academy adjourned at 10:15.

HEINRICH RIES,
Secretary of Section.

SUBSECTION OF ANTHROPOLOGY AND PSYCHOLOGY.

April 25, 1898.

President Osborn in the chair.

After some discussion, the section asked the chair to appoint a committee of four to confer with the council in reference to the number of meetings to be held by the Section of Psychology and Anthropology during the coming year. The committee appointed consisted of Messrs. Bliss, Farrand, McLouth and Boas.

The following papers were then presented:

J. D. Prince, Some Passamaquoddy Documents.

L. McWhood, A Method of Studying the Motor Effects of Music.

After the papers Charles B. Bliss was elected Secretary of the subsection for the coming year.

Charles B. Bliss,
Secretary of Section.

REGULAR PUBLIC LECTURE.

The third public lecture of the year was delivered Friday evening, April 29, 1898, at the Mott Memorial Library, by **James Douglass, Esq.,** of New York, on the subject: Fifty Years' Progress in Mining and Metallurgy in the United States.

The lecture was copiously illustrated by lantern slides, and was both descriptive and statistical. The changes in centers of production and the improvements in furnaces were shown for iron, copper and the precious metals. At the conclusion of the lecture there was passed a vote of thanks to Mr. Douglass.

Forty members and friends were present.

Richard E. Dodge,
Secretary.

STATED MEETING OF THE ACADEMY.

May 2, 1898.

Academy met at 8 P. M., Mott Memorial Library. President Osborn in the chair. In the absence of the Secretary, Mr. William Hallock was appointed Secretary, *pro tem.* Minutes of the last meeting were read and approved.

Members Elected.

The following candidates for election, approved by the Council, were read by the Secretary who was authorized to cast one ballot for them, which he did.

Dr. L. T. Chamberlin, 128 Fifth avenue.

Francisco G. P. Leão, 23 State street.

Charles E. Tripler, 121 West 89th street.

Amendments to By-Laws.

The following amendments to the by-laws recommended by Mr. C. F. Cox, Mr. Wm. Hallock and Mr. Edmund B. Wilson, a committee acting for the Council, were read and laid on the table for one month, in accordance with the rules:

1. That Section 2 of Chapter I of the by-laws be repealed.

2. That a new section be added to Chapter I, entitled Section 2, as follows:

"Any Resident Member or Fellow, who shall resign because of removal to a distance from the city of New York, may be restored to Membership or Fellowship at any time upon his own application, by a vote of the Council, and without payment of initiation fee."

A series of letters of acceptance were read by the Secretary from several of the new Corresponding Members.

Section of Astronomy and Physics then organized.

William Hallock,
Secretary, pro tem.

SECTION OF BIOLOGY.

MEETING OF MAY 9, 1898.

Professor Wilson in the chair, twenty-three persons present. The following programme was offered:

1. **C. L. Bristol,** MEASUREMENTS OF A LARGE LOBSTER CAUGHT OFF SANDY HOOK.

2. **H. L. Clarke,** NOTES ON BERMUDA ECHINODERMS, presented by C. L. Bristol.

3. **F. P. Keppel and G. N. Calkins,** REPORT ON THE HYDROIDS COLLECTED IN PUGET SOUND.

4. **E. B. Wilson,** ON THE STRUCTURE OF PROTOPLASM IN THE EGGS OF ECHINODERMS AND SOME OTHER ANIMALS.

5. In addition to the above Professor **Osborne** reported on some facts concerning a huge herbivorous Dinosaur, bringing out in particular the discovery of some hitherto unknown characters of the caudal vertebræ, and the peculiarly avian structure of the posterior cervical and the anterior dorsal vertebræ.

GARY N. CALKINS,
Secretary of Section.

SECTION OF GEOLOGY AND MINERALOGY.

MAY 16, 1898.

Professor Kemp in the chair. Ten persons present.

Minutes of the last meeting were read and approved.

Mr. **Geo. F. Kunz** exhibited specimens of Quartz crystals in massive Gypsum from Gallineo Springs, N. Mex., and announced the discovery of a new meteorite from Ottawa, Kansas.

The first paper on the programme was by Professor **D. S. Martin** on THE GEOLOGY OF COLUMBIA, S. C., AND ITS VICINITY. Professor Martin described the granitic and gneissic rocks of that region, and their residual products. He also commented on the character of the Potomac, Lafayette and Columbian formations which are well exposed in the railroad cuts south of the city.

The paper was discussed by Mr. Dodge and Dr. Ries.

The next paper of the evening was by Professor **Kemp,** entitled SOME REMARKS ON TITANIFEROUS MAGNETITES. The speaker discussed the formula of Ilmenite, and stated that it was probably a mixture of FeO, TiO_2, and *n* FeO_3. The amount of Titanium present in the titaniferous magnetites is very variable, running sometimes as high as 40% ; in the Adirondack ores it running 10–20%.

Magnetic methods of separation for the elimination of the Titanium have not yet proved successful. Nearly all of the titaniferous magnetites show small amounts of MnO, Cr_2O_3, CoO, NiO, V_2O_3 and MgO. The latter suggests the presence of Spinel. SiO_2 and Al_2O_3 have also been found, but Phosphorus and Sulphur are rare. Professor Kemp suggested that the rarer constituents might have some influence on the metallurgical behavior of the ore. The native and foreign occurrences of these ores were also alluded to.

Discussion of the paper was by Professor Martin, Dr. Ries and Mr. Kunz.

Owing to Dr. Ries' removal to Cornell University, his resignation as Secretary of Section was accepted, and Mr. Geo. F. Kunz elected Secretary for the remainder of the year.

Meeting adjourned at 10 P. M.

HEINRICH RIES,
Secretary of Section.

SECTION OF PHILOLOGY.

MAY 23, 1898.

The Section of Philology held its closing meeting for the year 1897 and 1898 on Monday evening, May 23d. The attendance numbered fifteen persons. Professor J. F. Kemp opened the session and presented Professor L. A. McLouth, the Chairman of the Section, who thereupon assumed the duties of presiding officer for the coming year.

Professor **T. R. Price** brought forward a contribution in

which he gave the results of his study of SHALL AND WILL IN LIVING ENGLISH USAGE. Dr. Price's investigations were confined to works that have appeared since 1850, in order to get the results of present usage. He chose as typical writings (1) a file of the London *Spectator* from August, 1897, to January, 1898; (2) The Poems of Stephen Philips; (3) The Essay of Henley on Robert Burns; (4) The Poems of Matthew Arnold; (5) The Idyls of Tennyson that have appeared since 1850. He presented only that part of his paper which dealt with the first person; the second and third persons are reserved to be printed. His results showed that *shall*, *should* are the normal usage in the first person; *I will* and *I would* in best usage are regularly confined to the idea of volition. The distinction seems to be quite sharply made in the best writers; and the number of occurrences is equally balanced. Several of those present took part in the discussion that followed.

The second paper of the evening was by Professor **L. A. McLouth,** and was entitled, NOTES ON E. JOSEPH'S KURENBURG THEORY. Dr. McLouth emphasized the strong points in Joseph's monograph, but criticised the tendency which the writer showed at times, it seemed, somewhat arbitrarily to reconstruct the text on the basis of a preconceived theory. Dr. McLouth favored rather a more conservative method.

Shortly after ten o'clock the meeting adjourned.

A. V. WILLIAMS JACKSON,
Secretary.

STATED MEETING.

JUNE 6, 1898.

Academy met at 64 Madison avenue, Vice-President Britton in the chair. Minutes of the last meeting were read and approved.

The changes in the by-laws which were to be brought up for adoption at this meeting were laid over until October, a legal quorum not being present.

After a notice by the Secretary about the meeting place for next year, the Section of Astronomy and Physics organized.

RICHARD E. DODGE,
Secretary.

SECTION OF ASTRONOMY AND PHYSICS.

JUNE 6, 1898.

Regular monthly meeting of the Section was held on Monday, June 6th, at 8 P. M., the chairman, Dr. P. H. Dudley, presiding. There were nine members and guests present.

The minutes of last meeting were read and approved.

Dr. **P. H. Dudley** read a paper on STRAP RAILS OF THE MOHAWK AND HUDSON RAILROAD, and showed a specimen of the rail, rolled in the year 1826.

After a few general questions and remarks, Dr. Dudley described the improvement that has been made in the condition of the track of the Boston and Albany Railroad, by the use of heavy rails, especially on steep grades.

Professor **D. S. Martin** then read a paper entitled, ARCHEOLOGICAL NOTES NEAR COLUMBIA, S. C., and showed specimens of curiously marked pieces of pottery found in that locality; also, a very interesting shell that had probably been used as a drinking cup.

After brief discussion, the meeting adjourned at 9:25 P. M.

R. GORDON,
Secretary.

REGULAR BUSINESS MEETING.

OCTOBER 3, 1898.

Academy met at 12 West 31st street, at 8 P. M., Vice-President Kemp in the chair. The minutes of the last meeting were read and approved.

Proposed changes in the by-laws in reference to Correspond-

ing and absent Members were referred back to the committee on by-laws, on request of the Secretary.

Section of Astronomy and Physics then organized.

RICHARD E. DODGE,
Secretary.

SECTION OF ASTRONOMY AND PHYSICS.

OCTOBER 3, 1898.

Section met on Monday evening, October 3, 1898, at 8 P. M., Vice-President J. F. Kemp in the chair. There were eighteen members and guests present.

The minutes of the meeting of June 6, were read and approved.

The Secretary then read a paper by Mr. **P. H. Dudley** on STREMMATOGRAPH RECORDS, giving some recent results obtained with the instrument under locomotives, and entire trains. Brief remarks were elicited by the paper, after which another by the same author was read by the Secretary, entitled OXYDATION OF RAILS IN TUNNELS. After a few remarks on the subject, the Section adjourned.

REGINALD GORDON,
Secretary.

SECTION OF BIOLOGY.

OCTOBER 10, 1898.

In absence of the chairman, Professor Wilson, Professor Osborn presided. Twenty-four persons were present.

Professor Osborn referred to the loss sustained by the Academy, and the Biological Sciences in general, through the death of Professor Baur, of Chicago, and of Dr. Arnold Graf, of New York.

Following the usual custom the meeting was devoted to accounts given by various members of their summer's work.

Professor **H. F. Osborn** described the different museums which he visited in Europe, giving a very brief account of the good and bad points of each. At Stuttgart he saw a unique and undescribed fossil *Hyrax* which Professor Fraas very generously gave him the pleasure of describing. The description was presented at the Meeting of the British Association in Cambridge.

Professor Osborn was followed by Professor **N. L. Britton,** who gave a resumé of the work accomplished during the summer on the building and grounds at the Botanic Garden in Bronx Park.

Professor **B. Dean** reported on a few results on the embryology of the Hag Fish, which he thinks is similar to that of the sharks. He also described the appearance of a South American Lung Fish (*Protopterus*) which was sent to him in a ball of dried mud.

Dr. **O. S. Strong** and Mr. **H. E. Crampton** reported briefly on the nature of the work accomplished at the Marine Biological Laboratory at Wood's Holl, bringing out particularly the fact of the cordial relations between the investigators of the Fish Commission and those of the laboratory.

Mr. **N. R. Harrington** related some interesting experiences in connection with his expedition to the Nile valley in quest of *Polypterus bishir*. The expedition, which was made possible by the generosity of Mr. Chas. H. Senff, was undertaken by Mr. Harrington and Dr. Reid Hunt. As guests of the Egyptian government they enjoyed unusual advantages in securing their ends, but only after repeated trials and discomforts and many disappointments did they finally get the fish.

Other brief reports were made by Professor Lloyd (on the botanic gardens of Germany), Dr. Brockway and Mr. Calkins.

At the suggestion of Professor Osborn and Dr. Dean a series of nominations for corresponding membership was sent to the Council.

GARY N. CALKINS,
Secretary.

REGULAR MEETING.

October 17, 1898.

Academy met at 8 P. M., Vice-President Kemp in the chair. Twenty-five persons present. In the absence of the Secretary, reading of the minutes was dispensed with.

Members Proposed.

The following nominations for new members were presented for the Secretary by the chair :

Jacob M. Rich, 50 West 36th street; Ernest Foley, 108 East 62d street.

The nomination of Dr. Henry S. Washington, of Locust, N. J., was made by Mr. George F. Kunz. These three names were referred to the Council under the rules.

The following paper was read by title: Annotated Catalogue of the Family of Muricidæ North of the Isthmus of Panama, by **Frank C. Baker,** Chicago.

Section of Geology and Mineralogy then organized.

Richard E. Dodge,
Secretary.

SECTION OF GEOLOGY AND MINERALOGY.

October 17, 1898.

Section met at 8 P. M., Professor Kemp in the chair, and twenty-two members present.

The first paper, by Professor **J. F. Kemp,** on the Minerals of the Copper Mines at Ducktown, Tenn., gave a brief history of the mines, and described some of the processes employed in treating the ores and the character of the rocks and the associated minerals. The paper was illustrated with an extended series of lantern views of the mines and the works, and with a suite of specimens. Professor Kemp referred particularly to the extremely interesting crystals of Almandite Garnet which he showed, in which the faces of the hexoctahedron are strik-

ingly developed, giving 48-sided forms, sometimes with small faces of the rhombic dodecahedron in addition. Zoisite also occurs in fine terminated crystals, and Limonite of remarkable iridescence.

The second paper, by Dr. **Arthur Hollick,** was entitled NOTES ON THE GLACIAL PHENOMENA OF STATEN ISLAND, and embodied the general results of several years of study and exploration by himself and others. The author outlined the topography of the island and the distribution of drift material upon it, and described the transported contents of the drift with relation to their sources. Most of the drift material is made up of the Triassic sandstone and shale from the adjacent mainland, ground up by the ice-sheet; but the boulders are largely brought from afar. They comprise material from all the fossiliferous beds of central New York, from the Potsdam to the Hamilton, but there is a great preponderance of Lower Helderberg and Schoharie grit. The fossils are in many cases finely preserved, have been collected in large quantities, very carefully studied and determined. The question as to the route by which they have come, over the hilly and almost mountainous regions lying between their source and their resting place is one of much interest.

An extended discussion followed the reading of this paper. Mr. van Ingen claimed that the course had probably been down the Mohawk Valley to that of the Hudson and then down the latter, rather than over the highlands of southern New York. Professor Stevenson suggested that the transportation over this long distance may have been due to repeated glacial movements, each transporting over a moderate distance.

The next paper was by Mr. **Francis C. Nicholas,** on the SEDIMENTARY FORMATIONS OF NORTHERN SOUTH AMERICA, and dealt with a large area of little-explored country between the Caribbean coast and the Northern Andes. It was illustrated by a most extensive and carefully labeled series of rocks, ores and minerals from many localities and horizons, to which it was impossible to do justice within the limits of the evening. Among many interesting points described and illustrated with specimens was the agency of sun's heat as a rock-disintegrator;

the changes of day and night temperature in high regions in the tropics producing a fracturing of the superficial portions of exposed rocks, comparable in result to the action of frost in higher latitudes.

The last paper was by Mr. **Geo. F. Kunz,** upon A METEORIC STONE THAT FELL AT ANDOVER, MAINE, ON AUGUST 5, 1898, with exhibition of the stone, or rather about half of it. The fall took place early in the morning of a cloudy and threatening day, so that the sound made by the meteor, which was heard for many miles around, was generally supposed to be thunder. A dark cloudy trail, like a dense smoke, followed and marked the path of the body through the air. Its course was from the north, southward, and in coming down it tore its way through a group of large trees, struck a heavy stone in a wall near the ground and buried itself in the earth. Here it was found two days later, by that time entirely cooled. The specimen is a typical stony meteorite, with a thin black crust on the outside, and of a bright pale gray on the broken surface, with very little iron. It weighs about 7 pounds, and its description will appear, later.

GEO. F. KUNZ,
Secretary.

REGULAR MEETING.

OCTOBER 24, 1898.

Academy met with President Osborn in the chair. Reading of the minutes was dispensed with.

MEMBERS PROPOSED.

The following nominations for Resident Membership were read by the Secretary and referred to the Council under the rules:

MATURIN L. DELAFIELD, JR., 56 Liberty street.

R. ELLSWORTH CALL, 201 Lenox avenue, Flatbush, Brooklyn.

After a notice from the President in reference to the forthcoming meeting of the American Society of Naturalists, the Section of Psychology and Anthropology organized.

RICHARD E. DODGE,
Secretary.

SECTION OF ANTHROPOLOGY AND PSYCHOLOGY.

October 24, 1898.

At the close of the regular meeting of the Academy, the Section of Anthropology and Psychology organized by appointing Professor Osborn Chairman *pro tem.*

Professor **J. McKeen Cattell** presented a paper entitled Some Anthropological Tests and Measurements, showing two new instruments. Reports of summer field work in anthropology were then made by Dr. **Livingston Farrand** and Mr. **Harlan I. Smith,** who spoke of their work on the northwest coast, and by Dr. **H. M. Saville** and Dr. **Carl Lumholtz,** who gave an account of explorations in Mexico.

Charles B. Bliss,
Secretary.

PUBLIC LECTURE.

October 31, 1898.

The first public lecture of the season of 1898–99 was given by Professor **George W. Blodgett,** of the Boston and Albany Railroad, on Railway Signalling, Past and Present. The lecture was under the auspices of the Section of Astronomy and Physics.

The lecturer was introduced by the Chairman, Mr. P. H. Dudley, who gave a brief summary of railway progress within the last few years.

Professor Blodgett spoke simply and very interestingly for an hour and a-half, sketching the various systems of railway signals in use on the more important railroads, and illustrating his remarks with an extensive series of well-chosen lantern slides. The lecture was free from technicalities, and very pleasing.

About sixty guests were present, and at the close of the lecture a vote of thanks was unanimously extended to Professor Blodgett.

Richard E. Dodge,
Secretary.

REGULAR MEETING.

November 7, 1898.

Academy met with Mr. P. H. Dudley presiding.

There not being a quorum present, the business meeting was postponed to Monday, November 14th.

Richard E. Dodge,
Secretary.

SECTION OF ASTRONOMY AND PHYSICS.

November 7, 1898.

Stated meeting, Monday, November 7, 1898, Dr. P. H. Dudley presiding. Eight members present.

Professor **J. K. Rees** read a paper on Variation of Latitude and the Constant of Aberration. In this he explained the scope of the work that had been done in this direction at Columbia University during the years 1894–98, gave a summary of the results, and stated that in future these observations would be carried on chiefly at Government observatories. Accompanying the paper were plotted curves to show the displacement of the earth's axis from time to time, based upon these observations.

The Section then adjourned.

R. Gordon,
Secretary.

ADJOURNED BUSINESS MEETING.

November 14, 1898.

Academy met at 8 P. M., President Osborn in the chair. Reading of the minutes of the previous meeting was dispensed with.

Members Elected.

The following names for membership were reported from the Council, and the Secretary was instructed to cast one ballot for the list, and they were thereby elected.

M. L. Delafield, Jr., 56 Liberty street. Life Member.
Ernest Foley, 108 East 62d street.
Dr. H. S. Washington, Locust, N. J.
Jacob M. Rich, 50 West 38th street.
R. Ellsworth Call, 279 Winthrop street, Flatbush, Brooklyn.

Members Proposed.

The following nominations for membership were made and referred to the Council :

Rev. A. B. Kendig, 86 Vernon street, Brookline, Mass.
Daniel C. Beard, 204 Amity street, Flushing, L. I.
B. Talbot B. Hyde, 82 Washington street. Life Member.
J. D. Irving, Columbia University.
Professor Graham Lusk, New York University Hospital and Medical College.
Marshall A. Howe, Columbia University.
Dr. L. H. Reuter, Merck Building, New York City.
Mason A. Stone, 20 East 66th street.
M. H. Beers, 408–410 Broadway.
Dr. Ivan Sickels, 17 Lexington avenue.
Alfred Douglas, 170 West 59th street.
Dr. Max Meyer, 159 West 103d street.
William L. Mason, 170 Fifth avenue.
William Wicke, First avenue and 31st street.
Edward R. Hewitt, 119 East 18th street.
Professor Charles H. Judd, New York University.

A series of proposed by-laws, presented by the Council, were read by the Secretary, and laid on the table until the next business meeting, according to the rules.

Richard E. Dodge,
Secretary.

SECTION OF BIOLOGY.

Meeting of November 14, 1898.

Sixteen persons present.

The resignation of Professor E. B. Wilson was read and ac-

cepted by the section. Professor Frederick S. Lee was unanimously elected chairman of the section.

The following programme was then presented:

1. **H. F. Osborn.** On the Presence of a Frontal Horn in Aceratherium Incisivum Kaup.

2. **H. F. Osborn.** On Some Additional Characters of Diplodocus.

3. **W. D. Matthew.** On Some New Characters of Clænodon and Oxyæna.

4. **W. E. Ritter.** On the Ascidians Collected by the Columbia University Puget Sound Expedition of 1896. Presented by Dr. Dean.

5. **J. P. McMurrick.** Report on the Hexactiniæ of the Same Expedition. Presented by Dr. Calkins.

Professor **Osborn** described the appearance of an hitherto unrecognized frontal horn on the skulls of *Aceratherium incisivum* Kaup; a discovery of great importance as it practically removes *Aceratherium* from the group to which it gives its name and ranges it with the rhinoceroses. Professor Osborn suggested that it may possibly be an ancestor of *Elasmotherium*.

In discussing the paper Dr. Wortman criticised the common tendency to create types based an a single character, citing in support of his suggestion the considerable variations to which single individuals of a species are subject, and giving one or two instances where errors have occurred.

In his second paper Professor **Osborn** described the structure of the vertebræ of *Diplodocus*, bringing out in considerable detail the variations in the sacrum of the herbivorous Dinosaurs.

Dr. **Matthew** briefly described the characters of the teeth, manus and pes of *Clænodon*, a form belonging to one of the three families, Arctocyonidæ, which gave rise to the present-day Carnivora. The structure of the wrist bones in particular brings this form almost within the limits of the Carnivora and Dr. Matthew regards it as a primitive bear which lived on fruits, honey or other soft foods.

Oxyæna another typical Creodont, was also described by Dr. Matthew, the principal points brought out being the disproportion of the brain case, limbs and lower jaws.

In the discussion which followed, Professor Osborn showed that while *Clænodon* undoubtedly possesses many precocious bear-like structures there are many difficulties to be pushed aside before it can be considered the direct ancestor of the bear. There are transitional forms for example between dogs and bears, as shown in certain types of teeth (*Amphicyon*), while on the other hand there is a marked difference in the size of the brain of the Arctocyonidae and that of the bears; the brain of the former resembling more closely the brain of the marsupials. If the *Amphicyon* evidence is of a sufficient phylogenetic value the bear line must have arisen much later than Dr. Matthew believes.

Dr. Lee also questioned the advisability of ascribing particular functions to specialized structures, a criticism which Dr. Matthew met by saying that in this case the relation of structure to function was in the nature only of an hypothesis; an explanation supplemented by Professor Osborn's statement that in all such cases it is necessary to have some working hypothesis, although each hypothesis is considered merely tentative.

At the request of Dr. Dean, Mr. **Richard Weil** was asked to give the main results of his observations on the DEVELOPMENT OF THE OSSICULA AUDITUS IN THE OPOSSUM. Mr. Weil finds that both the malleus and incus are derived from the mandibular arch and have no connection with the hypidean, thus confirming the older German view.

The other papers on the programme presented by Dr. Dean and Dr. Calkins were strictly technical and received only brief mention.

GARY N. CALKINS,
Secretary.

REGULAR MEETING.

NOVEMBER 21, 1898.

Academy met with Vice-President Kemp in the chair.
Reading of the minutes was dispensed with.

Members Proposed.

The following nominations were read and referred to the Council :

Fred W. Franklin, 700 West End avenue.

John I. D. Bristol, 1 Madison avenue.

Rudolph Keppler, 28 West 70th street.

Academy adjourned.

Richard E. Dodge,
Secretary.

SECTION OF GEOLOGY AND MINERALOGY.

November 21, 1898.

Section met at 8 P. M., the Chairman, Professor Kemp presiding. Minutes of last meeting were read and approved.

The first paper of the evening was by Dr. **J. H. Pratt,** State Mineralogist of North Carolina, on the Occurrence, Origin and Chemical Composition of Chromite. An abstract follows.

Chromite has only been found in the peridotites and allied basic magnesian rocks and in the alteration products of these rocks. The mineral occurs in grains or crystals and in imbedded masses near the boundary of lenticular masses of peridotite that have been shown to be of igneous origin. The chromite occurs in the fresh as well as in the altered peridotite.

The theory advanced by the author for the origin of the chromite is that the mineral was held in solution by the molten mass of peridotite and crystallized out from the molten magma as this began to cool.

The fused mass of rock would hold the different minerals in solution, and as this began to cool, the minerals would separate out, not according to their fusibility but according to their solubility in the fused magma. The more basic minerals being the more insoluble would be the first to separate out and in the present case would be the minerals chromite, spinel and corundum. This crystallizing or solidifying out from the molten magma would take place first on its outer boundaries, for here

it would cool first. Convection currents would tend to bring new supplies of material to the outer zone where the chromic oxide would be deposited as chromite.

This theory would account for all the vagaries of chromite deposits, their pockety nature; the shooting off of apophyses from the main masses of the chromite into the peridotite, the widening and pinching of the chromite lodes; and the apparently non relation or connection of one pocket of chromite with another. The masses of chromite that are found in the midst of a peridotite formation, which at the present time are isolated and have no connection with each other, were at the time of their formation part of the chromite concentrated near the border of the peridotite.

In mining for either chromite or corundum it is in that deposit found near the contact of the peridotite with the gneiss that a large deposit of either of these minerals would be expected to be found.

Chemical Composition.—From an examination of the analysis of chromite it is shown that a nearly pure chromite, with the composition FeOAO, is rarely found in nature. With the exception of three, in all the chromite analyses examined, alumina and magnesia were invariably present, and this would seem to indicate that the molecule of the mineral now called chromite is not pure FeOAO, but is a combination of the three isomorphous molecules; $FeOA_2O_3$; $MgOA_2O_3$; and $MgOAl_2O_3$. The ratio of the $FeOA_2O_3$ to the $MgOA_2O_3$ or $MgOAl_2O_3$ is generally 8 to 10 : 1.

An analysis of a chromite from Webster, Jackson Co., N. C., gave as A_2O_3 — 95%; Al_2O_3 — 29.28%; FeO — 13.90, and MgO — 17.31. This gave for the formula of the chromite, ratio of the molecule $MgOA_2O_3$ observed in any of the chromite examined.

It was noticed that the magnesia usually varied with the alumina, those rich in alumina being correspondingly rich in magnesia.

The second paper was by Professor **D. S. Martin**, entitled NOTES FROM THE SEMI-CENTENNIAL MEETING of A. A. A. S.

Dr. Martin summarized the more important papers in geology given at the 1898 meeting of the Association, and particularly the papers devoted to glacial phenomena.

Section adjourned at 9:45.

G. F. Kunz,
Secretary.

SECTION OF ANTHROPOLOGY AND PSYCHOLOGY.

November 28, 1898.

Section met at 8 P. M., with President Osborn and the Secretary in charge of the meeting.

The following paper was read by title: A Paleozoic Terrane Beneath the Cambrian, by **Geo. F. Matthew,** of St. John, N. B.

The first paper of the evening was by Dr. **Geo. V. N. Dearborn,** entitled, The Emotion of Joy. Brief summary of a monograph in experimental and descriptive physiological psychology. "Somewhat in proportion to its pleasantness, an emotional extramotion of 'expression' consists in general expansiveness and outwardly in contraction of the extensor muscles; this is, in particular, true of the smile and laugh of joy, the muscles concerned in which, from the early fœtal cervical flexion are properly of the extensor sort." Four series of experiments (nearly 3,500 in number), on the hands, head, arm, and leg, prove the correlation between pleasantness and organic sensation. The regular occurrence of habitual inhibitions, due to the complex conditions of civilized social development, supplies the apparent deficiency of the kinæsthetic theory in case of the emotions of man. Human "emotions" are not so in the biologic sense, but rather concrete expressions of the affective social consciousness at present quite indefinite.

The second paper of the evening was by Mr. **E. G. Dexter,** entitled The Influence of the Weather on Mental Activities of Children, and was devoted to the particular study of the apparent influence of the weather on the children of Den-

ver, Col., as shown by the study of some 600 cases of punishment inflicted upon children during a period of years. It was illustrated by diagrams, and created considerable discussion.

The third paper was by Mr. **Stansbury Hagar,** entitled The Water Burial. Mr. Hagar paid particular attention to the evidences of water burial as seen among the Micmac Indians, and gave a brief survey of similar customs in all parts of the world, present and past.

The last paper was by Mr. **A. Kroeber,** entitled Remarks on the Eskimos of the Cumberland Sound. In this paper Mr. Kroeber compared certain tales of the Eskimos of Cumberland Sound with those of other Eskimos, and paid particular attention to two or three tales which were of unusual interest because of their variations from the ordinary myths as hitherto known among the Eskimos.

Section adjourned at 10 P. M.

Richard E. Dodge,
Secretary.

SUBSECTION OF PHILOLOGY.

November 28, 1898.

The meeting was called to order at 8:30 P. M., by the Chairman, Professor McLouth.

The first paper was by Professor **C. L. Speranza,** entitled Machiavelli. Machiavellism in the odious sense generally attributed to the word, is misleading and does great injustice to Machiavelli. It originated in the fact that no notice was taken of the great man's works except the one "Del Principe," which, moreover, was misunderstood and judged from the standpoint of morals instead of that of logic and science, as it ought. The great aim of the booklet, namely, the formation of a great Italian state, founded on the universal consent of the people, finding its legitimacy within itself, independent, autonomous, and defended not by mercenary soldiers, but by its own citizens, was lost sight of. All importance was attached to what immoral means the

author, prompted by experience, proposed as best fitted to obtain that aim ; and none whatever was given to the sound and, in some capital respects, original theory set down by him, according to which the ruler of a state must act exclusively as the representative of that state, propose to himself no other object than the good of it, ascertain the best means to accomplish it, and apply these means intelligently and resolutely. While Machiavelli was convinced that the task of forming a great Italian state capable of preserving its independence could be carried out only by one man, and not by a republic, he was also convinced that it was for the people to consolidate and make fruitful the work performed by the one man. But the coiner of the word Machiavellism took no notice of this ; he ignored absolutely Machiavelli's "Discors," by which he taught the people how to govern themselves, and in which he devised the program of democratic government which is entirely modern. Nor was any notice taken of the other fact that Machiavelli proclaimed the necessity of an international code regulating the conduct and results of war, as well as other mutual relations between states ; or of the foundation laid by him upon which the philosophy of history has in modern times been built ; or of the thoroughly experimental method by which he arrived at his conclusions ; or the blow inflicted by him upon the artificial literary form of his days, and the inauguration of the ordinary, direct, natural way of discourse. In fact, Machiavellism, in its generally accepted significance, represents what in Machiavelli's system was merely transitory and dependent upon circumstances of place and time, instead of representing what was original, characteristic and of permanent value.

The second paper was by Professor **A. Cohn,** entitled Some Reforms in French Spelling. The needed reforms in French spelling are those that consist in introducing more uniformity, and correcting mistakes that have crept in through misapprehension. In the word *legs* (legacy), for instance, the *g* was introduced, in the sixteenth century, by grammarians who thought this word came from the verb *léguer*, while it really comes from *laisser* (to leave), a good reason for not pronouncing the *g*.

The most important reform needed is the substitution of *s* for *x* in the plural, words like *chapeaux*, and in masculine adjectives like *généreux*, and, in general, at the end of all words where *ux* is preceded by a vowel. The presence of the *x* in these words is the result of a misapprehension ; in old French texts the letter *x* is there for *us*, as shown by the interchangeable spellings in the same texts (for instance, *biax*, *biaus* are both found in *Aucassin et Nicolette*). We see thus that in the spelling *dieux*, the letter *u* is really twice represented. The advantage of spelling, in the plural *dieus*, *chapeaus*, and, in a whole class of adjectives, *généreus*, *odieus*, etc., is evident. Besides being a correction, it would simplify greatly the rules for the formation of the plural of nouns and adjectives, and of the feminine of adjectives, as well as the rules of pronunciation. The rule for the formation of the plural of nouns and adjectives in *au*, *eu*, *ou*, would then simply be the general rule : add an *s* to the singular. The rule for the formation of the feminine of adjectives like *généreus*, etc., would also be the general rule : add a mute *e* to the masculine.

Also why spell *nez* (nose) with *z*? This word comes from the Latin *nasum*, and in old French texts *z* stands for *ts*. Etymology would rather require to spell *nez* (Lat. *natos*) and *nés* (Lat. *nasum*), but, of course, no one thinks of substituting *nez* for *nés* in the participle.

Silent penultimate letters like *p* in *corps*, *temps*, might be dropped, and one might also spell *chanbre* instead of *chambre*, substituting *n* for *m* before *b* and *p*, a spelling that would bring more uniformity in the representation of nasal sounds. The Latin origin of these words would be just as clear to scholars as before.

None of these reforms, however, ought to be considered necessary, except the substitution of *s* for *x*, as above outlined.

This last ought to be introduced at once, for the present spelling is perfectly absurd. This paper was discussed by Professor Jackson.

Professor **E. G. Sihler** then read the third paper, on THE MAIN LINES OF CICERO'S POLITICAL JUDGMENTS. Dr. Sihler

was led to comment upon Mommsen's attitude toward Cicero and he endeavored to show from history and from Cicero's writings that the Roman orator's judgments of Cæsar were absolutely fair. Professor Sihler went on to show that Cicero actually had a definite policy, that he put himself on the conservative side as opposed to the tribunal or democratic party, and that such were his ideals and such the true convictions that he lived up to in his career. The paper was discussed by Professor Cohn.

The subsection then adjourned.

A. V. Williams-Jackson,
Secretary.

REGULAR BUSINESS MEETING.

December 5, 1898.

Academy met at 8 P. M., President Osborn in the chair.

Minutes of the last meeting were read and approved.

The following list of nominations were submitted from the Council, recommended for election as resident members, and the Secretary was authorized to cast a ballot for the list and they were thereby elected :

Resident Members Elected.

Rev. A. B. Kendig, 86 Vernon street, Brookline, Mass.

Daniel C. Beard, 204 Amity street, Flushing, L. I.

B. Talbot B. Hyde, 82 Washington street. Life member.

J. D. Irving, Columbia University.

Graham Lusk, New York University Hospital and Medical College.

Marshall A. Howe, Columbia University.

Dr. L. H. Reuter, Merck Building, New York City.

Mason A. Stone, 20 East 66th street.

M. H. Beers, 408–410 Broadway.

Dr. Ivan Sickles, 17 Lexington avenue.

Alfred Douglas, 170 West 59th street.

Dr. Max Meyer, 159 West 103d street.
William L. Mason, 170 Fifth avenue.
William Wicke, First avenue and 31st street.
Edward R. Hewitt, 119 East 18th street.
Charles H. Judd, New York University.
Fred W. Franklin, 700 West End avenue.
John I. D. Bristol, 1 Madison avenue.
Rudolph Keppler, 28 West 70th street. Life member.

The proposed by-laws, submitted to the Academy for adoption, were adopted with two slight amendments, and will appear printed in ANNALS, Vol. XII, No. 1.

After certain announcements by the Secretary in reference to new plans, the Academy adjourned.

RICHARD E. DODGE,
Secretary.

SECTION OF ASTRONOMY AND PHYSICS.

DECEMBER 5, 1898.

The meeting was called to order at 8:15 P. M. by the Chairman, Mr. P. H. Dudley; 24 members and guests being present. The minutes of the last meeting were read and approved.

Mr. **Wallace Goold Levison** presented a paper A SYSTEM OF CLASSIFICATION OF THE FLUORESCENT AND PHOSPHORESCENT SUBSTANCES, in which he classified as phosphorescent all those substances that give out rays of shorter wave-length than that of the rays they have previously received; and as fluorescent, all those substances that give out rays of greater wave-length than those they have received. The system was amplified by an arrangement of substances under headings with reference to the circumstances under which they phosphoresced or fluoresced. The classification was very clearly shown by lantern slides of charts on which all phosphorescent and fluorescent substances were set down, and in addition, remarks about their behavior under various circumstances. This classification has required much labor for its preparation, and at the conclusion

of the paper the members of the Section expressed their appreciation of it in a few remarks, with especial reference to the logical arrangement of the subject-matter.

There being no further business, the Section adjourned at 9:50 P. M.

R. Gordon,
Secretary.

SECTION OF BIOLOGY.

December 12, 1898.

Thirty-one persons present, Professsor Lee in the chair. The following programmé was offered :

1. **F. S. Lee.** The Course of Muscle Fatigue.
2. **W. K. Brooks.** The Embryology of Lucifer.
3. **F. E. Lloyd.** Studies in the Embryology of the Rubiaceæ.
4. **N. R. Harrington** and **Edward Leaming.** The Reaction of Amœba to Light of Different Colors.

Professor **Lee** showed that in the different types of animals studied by him in determining the course of muscle fatigue, the height of the curve, which represents the lifting power, becomes less and less in all cases. The reduction in height of the curve is accompanied in the case of muscles from the frog and turtle, by a concomitant increase of the duration of relaxation. The duration of contraction is also increased slightly in the frog and greatly in the turtle. In the cat neither of these secondary phenomena is represented, the height of the curve, or the lifting power, alone varying. The experiments show that the diminution of the latter phenomenon is the essential element in fatigue.

Professor **Brooks** gave a brief review of his interesting observations on the development of *Lucifer* bringing out in particular the essential features of cleavage and gastrulation which distinguish this decapod from most of its allies.

In the discussion which followed the paper it was shown by Professor Brooks that his results on the unusual mode of

cleavage of this form throw no light upon its phylogenetic position or upon that of its allies.

Professor Brooks' paper was accompanied by a demonstration of three microscopic preparations.

Professor **Lloyd** showed that in a number of genera of Rubiaceæ studied by him the embryo-sac is divided into two regions; an upper region in which the pro-embryo is developed, and a lower part containing numerous nuclei of uncertain origin. The suspensor of the pro-embryo develops branches which act as haustoria, taking food from the endosperm. The latter in turn takes its food from the integument by means of cells specialized for food absorption.

Dr. **Leaming** showed that light of different colors acts strongly upon the activities of *Amœba proteus*. Certain colors (red, orange, yellow and green) accelerate the protoplasmic flow, while other colors (white, violet and blue) retard it. The apparatus was fully described and the experiments were repeated in part, before the Section.

GARY N. CALKINS,
Secretary.

SECTION OF GEOLOGY AND MINERALOGY.

DECEMBER 19, 1898.

Section met with Professor J. F. Kemp in the chair. Twelve persons present.

A paper was read by Mr. **Henry S. Washington,** on THE IGNEOUS ROCKS OF ESSEX COUNTY, MASS. The rocks were described in some detail, and shown to be mainly Granites, Quartz-Syenites corresponding to the Akerites and Nordmarkites of Brögger; Quartz-Diorites and Diorites, with smaller areas of Nepheline-Syenite, Syenite, Essexite and Gabbro. These are cut by numerous dykes of various kinds, including Aplites, Granite-Porphyries, Paisanites, Solvbergites, Tinguaites, and many basic dykes, most of which are of Diabase, but some of camptonitic rocks. There are also extensive flows of Rhyolite,

accompanied by ash beds and breccias. Twenty-two analyses of the various types were given.

The character of the region as a petrographical province was discussed at some length. Chemically it was shown to be rich in alkalies, especially Soda, low in Lime and very low in Magnesia, and rather acid. The low Magnesia was commented on, and the occurrence noted of many minerals in these rocks as varieties poor in this oxide which are usually rich in it, as Lepidomelane, Fayalite and Glaucophane. The usually high ratio of FeO to Fe_2O_3 was discussed and it was pointed out that in most of the rocks FeO is extremely high, replacing MgO, while in the foyaitic group it is much lower. Iron oxides tend to vary with soda. Soda is constantly higher than Potash, but the molecular ratio varies a great deal, being about 1.10 in the granitic rocks, higher in the foyaitic group, and very high in the basic, the ratio in nearly every case approximating to whole numbers. This differentiation of Na_2O was commented on and its importance pointed out.

Comparisons were instituted with other regions and the great resembance to the rocks of southern Norway were described. It was shown that probably the chemical composition of the magma as a whole approaches that of a Nordmarkite, and that it is rather acid, as in Norway. The relations of the rocks of Essex county to those of the other alkali-rich regions of the Atlantic slope were also discussed.

The paper was discussed by Professor Kemp and others.

A. A. JULIEN,
Secretary pro tem.

General Index to Volume XI.

Names of authors in **heavy face type.**

Generic and specific names in *italics.*

Annals New York Academy of Sciences. Vol. XI.

INDEX SLIPS.

These index slips are furnished for two purposes:

1. To furnish the materials for a catalogue, the entries being cut out and pasted on cards.

2. To form the basis of both the index of the Annals of the New York Academy of Sciences and of a subject-catalogue.

Under the author's name is given the full title and reference. Then follow cross references to the important topics discussed. Minor references are limited to additions to scientific knowledge.

Cryptoscope of Professor Salvioni.
C. C. Trowbridge, 1898.
Annals N. Y. Acad. Sci., Vol. XI, p. 39.

Skiascope of Professor Magie.
C. C. Trowbridge, 1898.
Annals N. Y. Acad. Sci., Vol. XI, p. 40.

Lloyd, F. E.—On Hypertrophied Scale-Leaves in Pinus ponderosa.
Annals N. Y. Acad. Sci., Vol. XI, pp. 45–54; Pl. I, 1898.

Hypertrophied Scale-Leaves in *Pinus ponderosa.*
F. E. Lloyd, 1898.
Annals N. Y. Acad. Sci., Vol. XI, pp. 45–54; Pl. I.

Pinus ponderosa, Scale-Leaves of.
F. E. Lloyd, 1898.
Annals N. Y. Acad. Sci., Vol. XI, pp. 45–54.

Scale-Leaves of *Pinus ponderosa.*
F. E. Lloyd, 1898.
Annals N. Y. Acad. Sci., Vol. XI, pp. 45–54.

Pseudotsuga, compared with *Pinus ponderosa.*
F. E. Lloyd, 1898.
Annals N. Y. Acad. Sci., Vol. XI, pp. 40–51.

Attavism in *Pinus ponderosa.*
F. E. Lloyd, 1898.
Annals N. Y. Acad. Sci., Vol. XI, p. 51.

Hollick, Arthur. Notes on Block Island.
Annals N. Y. Acad. Sci., Vol. XI, pp. 55–88; Pls. II–IX, 1898.

Block Island [Rhode Island], Notes on.
Hollick, 1898.
Annals N. Y. Acad. Sci., Vol. XI, pp. 55–58; Pls. II–IX.

Determination of Stresses under moving Trains.
DUDLEY, 1898.
Annals N. Y. Acad. Sci., Vol. XI, pp. 89–116.

Railroad Rails, Stresses in.
DUDLEY, 1898.
Annals N. Y. Acad. Sci., Vol. XI, pp. 89–116.

Stresses in Rails under moving Trains.
DUDLEY, 1898.
Annals N. Y. Acad. Sci., Vol. XI, pp. 89–116.

Trains, moving, produce Stresses in Rails.
DUDLEY, 1898.
Annals N. Y. Acad. Sci., Vol. XI, pp. 89–116.

Howard, J. E., Railroad Track Experiments by.
DUDLEY, 1898.
Annals N. Y. Acad. Sci., Vol. XI, pp. 104–107.

WELLER, STUART.—Descriptions of Devonian Crinoids and Blastoids from Milwaukee, Wisconsin.
Annals N. Y. Acad. Sci., Vol. XI, pp. 117–126; Pl. XIV, 1898.

Devonian Crinoids and Blastoids from Milwaukee, Wis.
WELLER, 1898.
Annals N. Y. Acad. Sci., Vol. XI, pp. 117–126.

Crinoids from Devonian of Milwaukee, Wisconsin.
WELLER, 1898.
Annals N. Y. Acad. Sci., Vol. XI, p. 117.

Blastoids from Devonian of Milwaukee, Wisconsin.
WELLER, 1898.
Annals N. Y. Acad. Sci., Vol. XI, p. 117.

Milwaukee, Wisconsin—Devonian Crinoids from.
WELLER, 1898.
Annals N. Y. Acad. Sci., Vol. XI, p. 117.

Melocrinus nodosus, Hall.
WELLER, 1898.
Annals N. Y. Acad. Sci , Vol. XI, p. 118; Pl. XIV, fig. 6.

Melocrinus nodosus spinosus n. var.
WELLER, 1898.
Annals N. Y. Acad. Sci., Vol. XI, p. 119; Pl. XIV, fig. 2.

Melocrinus subglobosus n. sp.
WELLER, 1898.
Annals N. Y, Acad. Sci., Vol. XI, p. 120; Pl. XIV, fig. 1.

Melocrinus milwaukensis n. sp.
WELLER, 1898.
Annals N. Y. Acad. Sci., Vol. XI, p. 121; Pl. XIV, fig 7.

Melocrinus milwaukensis filosa n. var.
WELLER, 1898.
Annals N. Y. Acad. Sci., Vol. XI, p. 122; Pl. XIV, fig. 4.

Pentremitidea filosa Whiteaves (?).
WELLER, 1898.
Annals N. Y. Acad. Sci., Vol. XI, p. 122; Pl. XIV, fig. 3.

Pentremitidea milwaukensis n. sp.
WELLER, 1898.
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